THURSDAY, MARCH 20, 1873

PERCEPTION AND INSTINCT IN THE LOWER ANIMALS

THE correspondence in these columns, called forth by the letters of Mr. Darwin and Dr. Huggins (NATURE, Feb. 13), may be counted among the many indications of the growing interest in psychology; while at the same time it furnishes evidence of how far our knowledge of mind is behind most of the other sciences. Of the important points in the valuable letters of Mr. Darwin and Dr. Huggins we shall speak presently. But let us remark first on the minor and distinct question raised by Mr. Wallace. He says: "The power many animals possess to find their way back over a road they have travelled blindfolded (shut up in a basket inside a coach, for example), has generally been considered to be an undoubted case of true instinct. But it seems to me that an animal so circumstanced will have its attention necessarily active, owing to its desire to get out of its confinement, and that by means of its most acute, and only available sense, it will take note of the successive odours of the way, which will leave on its mind a series of images as distinct and prominent as those we should receive by the sense of sight. The recurrence of these odours in their proper inverse order—every house, ditch, field, and village having its own well-marked individuality—would make it an easy matter for the animal in question to follow the identical route back, however many turnings and cross-roads it may have followed." The objections to this hypothesis, to which Prof. Robertson has given his adhesion, are very serious. Let the scent of the dog be ever so acute, it is in many ways ill suited for supplying the kind of guidance required. A hound on the track of a hare has to follow a stream of the same scent. The association here is between the hare and the smell of the hare. Are not the associations of smell all of this kind? Is there any evidence that either in man or beast one smell ever coheres to another so as to render possible a memory of odours apart from the objects that give them forth? We are not very certain about the facts which the theory is put forward to explain; they are, however, better authenticated than is the fundamental assumption involved in the explanation. But, for the sake of argument, let us grant that a dog shut up in a basket can, as the result of a simple experience, link together several thousand smells in an unbroken series; say, the stink of a dung-hill is associated with the odour of sweet hay, this with the scent of a flock of sheep passed on the road, this again with the smell of a railway station to the right, and so on during a journey of sixty or seventy miles. If it be solely by the aid of this memory of smells that the dog is to return to the place whence it was taken, it must needs make haste back. It will be too late if the sheep have changed their position on the road. Especially is it necessary that it should get home while the wind still continues to blow in the same direction, otherwise its landmarks will be all in confusion. One other difficulty: suppose the dog to have got into the fragrance of the hay-field, which is perhaps forty acres in extent, how is it to find the dung-hill at the north-west corner? particularly if the wind be blowing the wrong way. Is it to scour round the ill-defined outskirts of the perfume until it comes on the ill smell of the dung-hill? If we try to conceive in terms of vision (we can make nothing of it from our experiences of smell) such a memory of smells as the dog is supposed capable of acquiring, we must represent to ourselves the sensations of being carried through a series of differently coloured mists, which, while they prevent us from seeing objects, blend and shade into one another. In such a case, though we might remember that the red came after the yellow, how, having got into the red, should we know in what direction the yellow might be found? These are among the difficulties that have not, it appears to us, been sufficiently considered by Mr. Wallace and Prof. Robertson.

But what are the facts to be explained? Such home-journeys of dogs as might, by a stretch of imagination, or perhaps more correctly, want of imagination, seem to be accounted for by the smell-hypothesis, rest only on a rather loose kind of evidence, which can be adduced quite as abundantly in support of performances to which this explanation can be in no way applicable. In returning home do dogs "follow the identical route" by which they were taken away? There is no evidence even of the second-hand, loose, hearsay description, that this ever happened in a single instance.* The general impression, on the contrary, is that they despise the windings of rivers, turnpikes, and railways, and make for their destination by the most direct route. For example, and to add one more to the thousands of stories, we may mention that since we sat down to write we have received a letter from a gentleman telling us that about fifty years ago his paternal grandfather, living at Quorn, near Derby, sent two hounds by coach to his maternal grandfather living at Liverpool. Two or three days after their arrival they absconded together; inquiries were set on foot, and it is said they were seen swimming the Mersey at a point a little above Liverpool, where the river is of great width. They could be traced no farther, but after some time they made their appearance at Quorn, "foot-sore and in bad condition." Again, sheep, pigeons, and other animals that have not the miraculous scent of the dog, are believed on as good authority to find their way home through strange regions and from equally long distances.

Alluding to this class of alleged facts, Mr. Spalding, in the February number of *Macmillan's Magazine*, ventured to favour the view that through all the turnings and windings of a long journey the creatures somehow retain a perception of the direction of the place from which they were taken, and he further ventured to think that a hint of a similar faculty is to be found in some men. In this connection the facts with regard to savages would be most valuable. What Mr. Darwin calls the "trifling fact," communicated in his letter of last week, namely that his horse, which had been sent from Kent, *viâ* Yarmouth, to Freshwater Bay, in the Isle of Wight, on the first day that Mr. Darwin rode him eastward, was very unwilling to return towards his stable, that every time Mr. Darwin slackened the reins "he turned sharply round and began to trot to the eastward by a little north, which was nearly in the direction of his home in Kent;" this observation, together with the circumstance that with the fact before his eyes, Mr. Darwin's "impression was that he somehow knew the direction whence he had been brought," appears

* See letter of "J. T." p. 384. We have other letters to follow.

to us very important indeed. In the present state of our knowledge of the subject a few such "trifling facts" are worth more than many volumes of ingenious speculation.

We come now to the more weighty question which formed the subject of Mr. Darwin's first letter. Is it probable that instincts have any other origin than transmission by inheritance of acquisitions resulting from what we call individual experience? We are here at the very outside edge of human knowledge, in a region where no prudent person would venture to speak with confidence. Indeed the mode of origin recognised in the question still appears a "wild theory" to such respectable representatives of educated opinion as the *Spectator*. Had it been our good fortune to know as matter of certain history that the well-marked instinctive antipathy towards butchers of the dog King and his descendants was originally produced by ill-treatment, we should have had evidence of the most positive and direct kind, that sometimes at least instincts do originate in this way. There seems no hope of getting such evidence in this particular case; and indeed it may well be that the instinct in question is much more ancient than Mr. Darwin seems inclined to suppose. It is however to be hoped that before long some lover of animals will try his hand at actually producing a new instinct. But while Mr. Darwin regards it as probable that most instincts are examples of inherited experience, he thinks it "almost certain that many of the most wonderful instincts have been acquired independently of habit, through the preservation of useful variations of pre-existing instincts. Other instincts may have arisen suddenly in an individual, and then been transmitted to its offspring, independently both of selection and serviceable experience, though subsequently strengthened by habit. The tumbler-pigeon is a case in point, for no one would have thought of teaching a pigeon to turn head over heels in the air; and until some bird exhibited a tendency in this direction, there could have been no selection." The authority of Mr. Spencer may be adduced in support of Mr. Darwin's position. He speaks of "the natural selection of *incidental* variations," and of feelings that cannot be referred to "the inherited effects of experiences." Nevertheless, let us look closely at this matter. Will Mr. Darwin's view bear to be stated in such a way as to express more than the fact that in a great many instances we cannot conceive how the instincts originated? Will it bear to be put in this form: that it is almost certain that many of the most wonderful instincts had their origin in useful variations or sudden conjunctions of psychical states of such a character as could not by possibility have any relation to the experiences either of the individual itself or of its ancestry? Anything short of this will, it seems to us, scarcely amount to the contention that instincts have a mode of origin distinct from experience and heredity. That some other factor of unknown power may work along with experience and heredity in producing instincts, we are not in a position to deny. But still less are we in a position to say that there is such a factor, or what that factor is, or to admit that it ever operates independently of experience and heredity. We do not know how the tumbling of the tumbler pigeon began. But suppose we were certain that we had witnessed the very first performance of this kind, and saw that it arose suddenly and without any assignable cause: What then? How did the tumbling begin? To call it an incidental variation is but a way—and, because to some minds it looks like an explanation, a bad way—of stating our ignorance. But could we say so much as that it was in no way connected with experience and heredity? We think not. This tumbling is a fancy instinct, an outlet for the overflowing activity of a creature whose wants are all provided for without any exertion on its part. And if we had before us the evolutional history of the pigeon we might be able to point to some long obsolete instinct or useful action and say, behold, when on the wing, the superabundant energy of the creature has burst along the old long disused but not obliterated tracks, and see the strange result.

This is the direction in which we think it would not be unscientific to look for an explanation, should we ever have any such facts to explain. A similar line of remark might be followed with respect to what Mr. Darwin calls useful variations of pre-existing instincts. The question is, whence these variations? Further, just in proportion as these variations are slight, must it be difficult to say that they are not connected with experience—with the experience of the individual. In pursuing this inquiry we should doubtless come on the question, What is meant by experience? Everybody, it may be said, surely knows that. Perhaps. It is, we think, probable that the discovery might be made that we have not very clear and well-defined ideas as to the exact nature, extent, and limits of what we call individual experience. Of course we cannot now enter on such an inquiry.

SEPULCHRAL MONUMENTS OF CORNWALL*

II.

Nænia Cornubiæ. A descriptive essay, illustrative of the Sepulchres and Funereal Customs of the early Inhabitants of Cornwall. By W. Copeland Borlase, B.A., F.S.A. (London: Longmans; Truro: Netherton, 1872.)

MR. BORLASE, assisted by a party of friends, early in 1872, opened two barrows on the summit of one of the most commanding elevations in the district, about a quarter of a mile east of Trevelgue or Trevelga Cliff Castle, near St. Columb Minor. The most westerly is 250 ft. in circumference, 11 ft. high at the centre, and its greatest axis, having an east and west direction, is 100 ft. At a depth of 2 or 3 ft. from the surface, the entire substratum, to the amount of several hundred cart-loads, was burnt earth, as red and almost as fine as brickdust. Beneath it and towards the eastern side was a cairn of stones about 12 ft. in diameter, and 4 ft. high. Many of them had been brought from the neighbouring beach, and were blackened by fire. Under this lay a large spar stone, such as does not occur in the district, singularly flat for a stone of the kind, measuring $10{\cdot}5 \times 5{\cdot}4 \times 1{\cdot}75$ ft., on a level with the surrounding country, and covering a chamber 6·16 ft. long from N.W. to S.E., 2·5 ft. broad, and 2·75 ft. deep. Its sides were formed of four slate stones, 7 or 8 in. thick, and set on edge, on each of which the covering stone rested. The floor seemed to have been paved with slates, but they had been displaced, and portions of an unusually thick human skull were found below them.

The eastern or more conspicuous barrow was 80 ft. in diameter, 13 ft. high, and had a depression of 1·5 ft. at

* Continued from p. 337.

the summit. Immediately under the turf was a bed of stones, 3 ft. deep, and lying on a stratum of hard clay brought from a neighbouring valley. It contained a few human bones and ashes without any protection, and was 5 ft. deep. Under this was a second layer of stones, 3 ft. deep; and lower still an immense slate stone on a level with the natural soil, and covering a vault measuring 5·17 ft. long from N.W. to S.E., 2·8 ft. broad, and 2·25 ft. deep. The bottom was a pavement 6 in. deep, on which lay a human skeleton on its left side. The head was at the north-east corner nearly a foot from the end wall; the legs were bent at the knees, the arms stretched out so that the hands must have, at least, nearly touched the knees, and the body was contracted into a length of 4 feet.

Mr. Borlase holds that the Menhirs, or "Long Stones," of which there are many in various parts of the county, were sometimes tombstones merely, sometimes memorials of important events, and that they ranged from a romote antiquity far down into modern times.

In the hope of throwing some light on their origin the author recently examined the ground immediately around them. The Pridden Stone, close to the farm-house of Pridden, in the parish of Buryan, near Penzance, is an extremely rude mass of granite, 11·5 ft. above the ground, nearly 20 ft. in girth where broadest, and tapering towards the top. Below the natural level of the ground, a shallow pit, covered with a flat stone a foot in diameter, contained a few splinters of human bones, charred wood, and a layer of burnt brownish mould, the whole not sufficient to fill a quart pot.

On the farm of Tresvennech, in the parish of Paul, near Penzance, stands a granite menhir on the summit of elevated ground 11·5 ft. above the surface, and 4 ft. below. Though unhewn it is tolerably square at the angles, symmetrical, and perfectly upright. In 1840, a farmer, working near it, accidentally struck his tool against a horizontal flat stone 18 in. square, beneath which was a pit cut out of the clay soil, having its sides unprotected, and containing an urn 19·4 in. high, and 14·3 in. wide at the mouth. It stood mouth upwards, and contained the larger fragments of the calcined bones, and a molar tooth, of a human body; whilst the smaller pieces, together with wood ashes, were scattered throughout the pit. The urn was hand-made, and consisted of yellow clay found in the vicinity. Its interior was hard and black, but the exterior was not well baked. The handles were remarkably large and neatly put on, but differed in shape and size. A smaller urn, 5·5 in. high and 4 in. wide at the mouth, found 18 inches from the former, was also standing on its base, but without protection of any sort. It was filled with snuff-coloured powder.

The Sepulchral Mounds, or Barrows of Cornwall, whether of earth or stones, range from 15 to 100 ft. in diameter, and from 2 to 25 ft. in height. They resolve themselves into Cone, Bowl, Bell, Flat, and Ring barrows, but there is no instance of the Long, Druid, Egg, or Twin form, *i.e.* two surrounded by the same trench. The author recognises two pretty well-defined varieties of Ring-barrow:—1. Where the stones stand *on end*, at some distance from each other, and enclose only a piece of *level* ground. 2. Where the stones are set *on edge* (rarely *on end*) contiguous to each other, and enclose either a large rock, a few small mounds, or an area of uneven ground.

Of the first kind, there are four distinct examples within a circuit of eight miles in the Hundred of Penwith. Mr. Borlase is of opinion that all circles of *erect* stones owe their origin to the same design which attained its perfection in Stonehenge, and that the only question remaining is how far they can be regarded as sepulchral in their origin or use. He remarks that whatever may have been their *origin*, history and tradition seem to point rather to a civil than a religious *use* of them, and in confirmation quotes the "Iliad" (xviii. 503), a passage thus translated by Mr. Wright:—

> "Heralds the people checked. Elders meanwhile
> On polished stones in sacred circle sate."

The *Ring Barrows* of the second variety range from 10 to 100 ft. in diameter. The author has little doubt that they were originally sepulchral, and that they are cairns or barrows in an incomplete or demolished state.

At Trigganeris, in the parish of Sancreed, on the top of a hill, are two erect stones 17 ft. apart in a direction from N.N.W. to S.S.E., 7·3 ft. and 9 ft. high respectively. Nearly midway between them, but entirely on one side of a line joining their centres, a grave, 6 ft. long in an almost east and west direction, 3·25 broad, and about 5 ft. deep, was found cut with much precision in the natural clay. It contained nothing but the disturbed fine subsoil of the neighbourhood.

In 1818, a grave 8 ft. long, 3·5 broad, and 3 high, was found under a pile of stones 30 yards in diameter, near the "Cheesewring," in the parish of Linkinghorn, in East Cornwall. The bottom was one long flat stone; each side consisted of three stones, each end of one, and it had a cover of a single stone. Within it lay the remains of a human skeleton extended, having near the breast an earthen pot, containing, according to the workman who found it, a golden cup. The vessels were covered and protected with a flat stone 16 inches square, which leaned diagonally over them against the west wall of the grave. In this grave the following relics were also found, but subsequently lost:—a small piece of ornamental earthenware, a bronze spear head 10 in. long, a metallic rivet, as is supposed, and a few glass beads. The gold cup has been preserved, and is 3·75 in. high, 3·375 in. in diameter at the mouth, 2·5 ounces in weight, and its bullion value is 10*l.* It is perhaps worthy of remark that before the discovery of the cup there was a tradition in the neighbourhood of a "golden boat" having been dug up in a stone cairn near the Cheesewring.

It does not appear necessary to attach a nautical meaning to the word *boat*, for, unless we are in error, vessels for containing melted butter, sauce, &c., were formerly termed *boats* in Cornwall, as well as elsewhere.

Before quitting this cup it may be as well to add that we have elsewhere seen 1837 mentioned as the date of the discovery, instead of 1818, as given by Mr. Borlase.

Well-authenticated instances of inhumation are stated by the author to be extremely rare in Cornwall, and amongst these only two or three examples of the extended position actually occur, whilst only one of the contracted position can really be cited. He inclines to the opinion that the British copied the practice of cremation at a period not earlier than that of their con-

tact with the Romans; and thinks that a very doubtful answer must be given to the question, "Have there been found in Cornwall any interments which can with some certainty be said to have preceded the practice of cremation?"

Mr. Borlase entirely dissents from the opinion that articles interred with the deceased had reference to any view of utility in the next world, or, in other words, that they are the result of a matured belief in a future state; but guards himself from being supposed to assert that such a belief was non-existent in the days of the barrows.

The large *sepulchral urns* are of two kinds. 1. *Vase-shaped* vessels from 10 to 20 inches high, ribbed round the upper part, sometimes ornamented with small indentations, but never with the chevron pattern. 2. *Barrel-shaped* vessels from 8 to 13·5 inches high, invariably ornamented with the chevron pattern.

Vessels differing from the foregoing in size only, and ranging from 4·5 to 6 inches in height, are very frequently found in Cornish barrows, and sometimes in close proximity to the large ones.

Mr. Borlase has compiled, from county histories, papers read to various societies, manuscripts of older antiquaries—especially Dr. Borlase—and his own note-book, a detailed account of explorations and discoveries in the Cornish tumuli, made at different periods, from Norden's account in 1584, of "an auntient buriall at Withiell," to his own researches at Pradannack in November 1871. It may be doubted whether any part of his interesting volume exceeds this in value; and we indulge the hope that others—not antiquaries only—may follow his example, and collect and record the researches that have been made in the departments to which they specially devote themselves, in the counties in which they reside. We should greatly like to linger over this chapter, but both time and space forbid us to do more—and this we do most cordially—than congratulate the author on the successes which have attended his explorations.

He devotes his last chapter to "The Age of the Monuments," and thinks that his discoveries justify the conclusion that some, at least, of the most typical of the interments might be brought within historic times, and assigned to the early centuries of the Christian era.

The facts which seem chiefly to delight him are those he discovered in 1863, on Morvah Hill, and of which he gives, as "most worthy," the following "second notice":—

"Here was an instance of an interment in a cairn, where the body had been burnt on a central natural work surrounded by the usual ring of stones, the ashes placed in an urn of the usual chevron pattern, accompanied by the usual limpet and flint, protected by the usual Kist-Vaen, and finally covered in by the usual pile of stones. The whole arrangement, in short, being one of the most typical examples of the generality of barrows opened in the district. But here, in the very Kist itself, what should appear but late Roman coins of the third century! What is the most natural inference, then? That the coins must be thrown out of the question, because of the flint chip? or the whole structure referred at once to the Stone Age, thousands of years B.C., because it is encircled by large stones, or because the pottery is rude, and its ornamentation not curvilinear? Is it not rather the only fair course to admit at once that this interment, although possessing every characteristic of the so-called Stone Age, was placed here not earlier than the third century, A.D., that is, at the time when the coin was struck?" (pp. 263-4).

Being ignorant, perhaps, of some of the facts of the case, we do not presume to say that the author has not correctly interpreted his case, but as he seems resolved to ignore the question of secondary interments, which have undoubtedly taken place in many instances, and for aught that appears, may have occurred at Morvah, we hold *non proven* to be at present the only safe verdict.

We cannot take leave of Mr. Borlase's work without thanking him for the numerous well-executed illustrations with which it is enriched, and congratulating the publisher on the manner in which it has been got up.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

New Experiments on Abiogenesis

HAVING occupied myself for some time past with an experimental study of abiogenesis, I have followed with much interest the controversies on that question in recent numbers of NATURE, and beg leave therefore to state to the readers of this journal the results of my experiments, which form in my opinion a not unimportant contribution to the solution of the interesting problem.

I fully agree with Prof. Burdon Sanderson in the confirmation of Dr. Bastian's statements regarding the well-known turnip-and-cheese experiments. A turnip decoction of the specific gravity 1·011—1·016, filtered and boiled with cheese (0·25—0·5 grm. to 50 c.c.), filtered again and neutralised, then boiled for 10 minutes and hermetically sealed, is after 2—3 days' exposure to a temperature of 30° C. swarming with *Bacterium termo*. It is however to be remarked that a too great concentration of the solution hinders the evolution of the Bacteria; the volume of the liquid employed should therefore not be too small, otherwise the boiling for 10 minutes will render the solution too much concentrated. Perhaps the negative results recorded by some experimenters are to be explained in this way.

Both turnip-decoction and cheese are a mish-mash of substances for a great part ill-defined and imperfectly known. It would be desirable to substitute for these materials some other mixture of better defined ingredients.

Instead of cheese can be used peptone (0·2 grm. to 50 c.c.) with the same result. The peptone is obtained by digestion of egg-albumen with artificial gastric juice, subsequently isolated and purified by repeated precipitation with alcohol.

In searching for a substitute for the turnip-decoction I found it available not to use the hermetically sealed vessels, but to experiment with free admittance of air. The recent explorations of Cohn, Burdon Sanderson, and Rindfleisch have shown that the germs of Bacteria are but sparingly present in common air, but notwithstanding it is a matter of course that in these experiments no other but perfectly filtered air can be admitted. After many trials the following mode of experiment was finally adopted.

I prepared a solution of the mineral salts, that are according to Cohn indispensable to the nutrition of Bacteria, viz. 1 grm. potassium nitrate, 1 grm. magnesium sulphate, 0·2 grm. calcium phosphate to 500 c.c. distilled water. (This mixture does not quite agree with the solution employed by Cohn. For various reasons, which will be stated elsewhere in a more extensive report of my experiments, I preferred this modification.) In 100 c.c. of this liquid were dissolved 2·5 grms. grape-sugar and 0·4 grm. peptone. About 50 c.c. of this solution was poured into a glass flask holding about 100 c.c. The mouth of this flask was beforehand polished flat, and a layer of hot molten asphalt laid on the flat brim. The liquid in the flask was then boiled for ten minutes over an ordinary Argand burner. The excessive frothing of the boiling liquid is easily prevented by using a small flame, and carefully regulating it when the froth is on the point of coming up, yet without allowing the liquid to

cease boiling. The ten minutes are reckoned from the moment the liquid begins to bubble freely.

The remarks of Dr. W. Roberts (NATURE, Feb. 20, p. 302) do therefore not apply to these experiments. Moreover, I observed frequently the temperature in the mouth of the flask, and found it always to be between 95° and 100° C. Therefore no part of the flask escaped the full effect of the germ-destroying heat.

While the boiling continued, a heated piece of unglazed flat earthenware tile was then pressed on the mouth of the flask; the solidified asphalt melted, and the piece of tile adhered, after cooling, firmly to the mouth of the flask. No air could then penetrate to the interior, except through the pores of the closing tile. The tile was 7 millimetres thick.

The previous heating of the tile is necessary, not only for melting the asphalt, but also for destroying the germs which possibly may adhere to the tile itself. This heating is effected in a Bunsen's burner, and raises the temperature of the tile to such an extent that a tuft of cotton-wool is scorched brown when pressed against it. The tile should not be too hot when it comes on the asphalt, lest the fumes arising from the decomposition of the asphalt should spread to the interior of the flask, and hinder the production of Bacteria.

The flasks prepared in this way were then exposed in a water bath to a temperature of 30—35° C. After two or three days the liquid was very turbid, very often a thick pellicle appeared on the surface, and flocky masses were swimming in the solution. The closing tile was then removed by melting the asphalt, and the microscopic examination showed myriads of *Bacterium termo* in lively motion, while the pellicle and the flocky masses consisted also of these organisms. *Micrococcus crepusculum* and *Vibrio serpens* were often present, but *B. termo* predominated. (The Bacteria were determined after Cohn's classification. "Beiträge zur Biologie der Pflanzen, II.")

The experiment conducted in this way yields always a positive result. Now three things are possible:—

1. The materials employed originally contained germs of Bacteria, which have simply developed themselves.

2. During the experiment germs have penetrated into the interior of the flasks.

3. The Bacteria have originated *de novo* in the liquid.

The first explanation is not admissible. For control-solutions treated exactly in the same manner, but composed in other proportions, remained under the same conditions perfectly free from Bacteria. These solutions were—

a. 100 c. c. of the above-named salt solution + 1 grm. ammonium-tartrate + 1 grm. grape-sugar.

b. 100 c. c. of the salt-solution + 1 grm. ammonium-tartrate + 0·2 grm. peptone.

Both these liquids are eminently suited to the nutrition and growth of Bacteria. That they nevertheless remained pure from these organisms proves that *none of the materials employed contained germs capable of resisting for ten minutes a temperature of* 100° C.

The second explanation is equally inadmissible. To prove this directly 1 grm. ammonium-tartrate was dissolved in 100 c.c. of the salt solution. This liquid was equally divided in two flasks, A and B. To A was added a trace of air-dust, collected in a little room where putrefying liquids were often standing, and then the flask was closed with a piece of tile in the above-described manner. B was boiled and closed as usual; but on the upper surface of the closing tile a considerable quantity of the same dust was loosely strewn. After twenty-four hours A becomes turbid, and swarms on the third day with Bacteria; B is on the eighth day still perfectly clear, and is then no longer examined. The conclusion is obvious: no germs of Bacteria do pass through the pores of the tile.

The only remaining explanation, is, in my opinion, this: under the above-described circumstances, Bacteria can arise without pre-existing germs. Not in any single case have I seen any other organisms than Bacteria—never fungi.

A certain concentration of the liquid is an important desideratum in these experiments. A specific gravity of about 1·012 is the most favourable. Greater dilution is a hindrance as well as greater concentration—at least when the above-named materials are employed. It is, however, not absolutely necessary to employ grape-sugar for raising the specific gravity up to 1·012. Common salt can do this just as well. Thus the following mixture is equally sufficient for generating Bacteria:—100 c.c. of the above-named salt solution, 2 grm. common salt, 0·2 grm. grape-sugar, 0·4 grm. peptone.

The first thing to be done now is to substitute for the grape-sugar and the peptone less complicated bodies. My experiments have been continued with this purpose.

This brief abstract may suffice for the moment. Shortly a more detailed communication and discussion will appear in one of the special journals. D. HUIZINGA,

Professor of Physiology at the University of Groningen

Groningen, March 15

The Janssen-Lockyer Method

SINCE my letter which appeared in NATURE of February 20 there has been a letter from Dr. Huggins (also in NATURE, February 27, p. 320), and I see that Mr. Richard A. Proctor has likewise published a letter in the *English Mechanic* of March 7.

With respect to the former of these communications I have but one remark to make. I was ignorant that the domestic bereavement to which Dr. Huggins alludes occurred at the time when the eclipse reports from India reached this country. This circumstance undoubtedly explains why Dr. Huggins did not sooner make the observation alluded to; and had I known the coincidence in point of time between these two events I should not have made the remark to which he refers.

Mr. Proctor's letter certainly surprises me, especially as coming from one who holds a prominent official position in the chief astronomical society of this country.

1. In the first place I cannot understand what Mr. Proctor means when he says with allusion to the question proposed by Mr. Lockyer in his preliminary paper of 1866—"I have always judged from the form of the query that, as he now mentions, Dr. Stewart had suggested its wording." If Mr. Proctor will refer to my letter in NATURE of February 20 he will find it stated that I advised Mr. Lockyer to introduce his views in the shape of a question, which he accordingly did. The wording was Mr. Lockyer's own, being the result of his own cogitations on the subject, and all that I did was to suggest the putting of it in the form of a question.

2. Nor can I understand what Mr. Proctor means when he says—"I can admit very readily that Mr. Lockyer clearly recognised the principle of the method for spectroscopically studying the prominences when he asked the query. I do not indeed see how any person at all familiar with spectroscopic analysis could have failed to do so, after reading Dr. Huggins' account of his observations of T Coronæ."

When Mr. Lockyer suggested the application of the spectroscope to the sun's red flames he knew, no doubt, and made use of his knowledge, that in white solar light the spectrum is scattered, while in light from incandescent gases it is not; but his information on these points was not surely derived from Mr. Huggins. Was not Newton the first to show that a slit of white light is dispersed by refraction into a broad band or ribbon? I do not know whether Newton ever clearly enunciated that in consequence of this dispersion the ribbon was less luminous than the slit. Perhaps he thought that this was sufficiently understood, but at any rate he who after Newton first made this announcement cannot be said to have made a very startling discovery. I have the highest respect for the brilliant discoveries of Dr. Huggins, but I am quite sure that he does not claim as one of these the statement that "when the feeble light of a nebula is dispersed into a spectrum consisting of light of all refrangibilities, the spectrum is extremely faint."

In like manner the man of science who first showed that incandescent gases give out only a few spectral lines made a great discovery; while he who after this discovery first announced that such light will not be weakened by dispersion can hardly be said to have made a discovery at all.

Now these two discoveries of two different kinds of spectra have been most prolific. Swan has used them; Kirchhoff has used them in certain experiments of his in which the conditions were probably very similar to those in T Coronæ, and after Kirchhoff, Huggins, and after Huggins, Lockyer. Each of these and many more have applied those well-known principles in many ways, none of the observers claiming the principles, but each one claiming his own application, being at the same time perfectly willing to acknowledge his neighbour's just claims. For instance, Dr. Huggins says—"To Mr. Lockyer is due the first publication of the idea of the possibility of applying the spectroscope to observe the red flames in sunshine." Now this is precisely the state of the case, and I need not say anything more.

3. Then as to the advice to Mr. Lockyer to put his suggestion in the shape of a query, it ought to be remarked that it was a

preliminary paper, and that Mr. Lockyer intended to follow up and did follow up his suggestion with the requisite observations. But at that time neither Mr. Lockyer nor myself knew to what extent the spectroscope would throw light upon these red flames. What service I did was in stating my opinion that those red flames were probably gases, while the suggestion as to how to detect them was due to Mr. Lockyer. Had the red flames proved to be solid particles, the spectroscope would have afforded only meagre negative evidence of their existence.

Our ignorance on this point suggested to us the propriety of a query. It was only in this respect that the query was ambiguous, that is to say there was no doubt that the method suggested by Mr. Lockyer would throw a great deal of light upon the red flames if they proved to be gaseous, but the only doubt in our minds was whether or not they were gaseous.

4. I have tried as well as I can in justice to both of us, to remember and reproduce what took place in the conversation in 1866 between Mr. Lockyer and myself, and to show the reasons for the form in which Mr. Lockyer's suggestion was put. Probably Mr. Huggins announced it afterwards in a more complete form, and probably it has since been announced in a yet more clear and complete form, if this be possible. For we know that as a rule discoverers and inventors have no great power of expression, and if the prize is to be given for the clearest possible utterance of a truth, it will be very seldom won by the discoverer, but will very frequently be obtained by the popular writer. Nevertheless I fail to see that Mr. Lockyer's original query was at all ambiguous in the sense that Mr. Proctor suggests. How, I should like to know (adopting the words of the query), could the spectroscope afford us evidence of the existence of the "red flames at other times than those of a total eclipse, unless by dispersing the reflected light."

5. Mr. Proctor asks, To what end are we to inquire whether Mr. Lockyer would or would not have detected the lines without the information derived from the eclipse observations? As far as I can understand the question I quite agree with Mr. Proctor. I do not think the inquiry ought ever to have been made. When, however, it was suggested that Mr. Lockyer derived aid from the Indian observations it was surely allowable for him to deny the imputation.

6. I have sometimes thought that discussions too frequently arise from the attempt to compare together the labours of different men, when all that is necessary is a statement of facts and dates. It is a matter of fact that the Indian observers at a particular date and under particular circumstances, made certain observations and derived certain results. It is likewise a matter of fact that Mr. Lockyer, at a date slightly later and under certain other circumstances, made observations of another kind, from which he also derived certain results. Different minds are differently constituted; one will think more of the Indian observations, another of those of Mr. Lockyer. But why should the two be compared together, unless some good object is to be gained by the comparison?

7. Now I cannot see why Mr. Proctor in his letter should have made the comparison at all, but since he has done so, I must be allowed to object to his method of making it. "Surely," he says, "on this matter we must assign Mr. Lockyer only the credit arising from the fact that, possessing an instrument which made the work child's play, he saw the lines by the method described by Huggins nine months before in detail, and depending on a principle which Huggins had stated fully two years and nine months before." I have already endeavoured to dispose of the latter part of the statement, and indeed Mr. Huggins has disposed of it himself; but with regard to the first part of it, I conceive that the possession of an instrument which made the work child's play, forms one element of the credit due to Mr. Lockyer. At first his instrument was defective and unable to do what he wished, but his conviction that something was to be made out of the sun, if examined by a powerful spectroscope, was so strong, that after much delay on the part of the optician employed, and sundry other discouragements, he at last procured for himself an instrument with which he saw the red flames at the very first trial.

8. I should not have alluded to the concluding paragraphs of Mr. Proctor's letter were it not written by one who holds a high official position in the Royal Astronomical Society. To my mind it is most deplorable that the secretary of this society should conceive himself at liberty to make in a public journal unsupported statements reflecting discreditably upon a distinguished member of that society. It is bad enough when two private men of science abuse each other, but it is not to be tolerated when a high official of a society of standing descends into the arena. If he insists upon doing so it is surely not unreasonable to request that he will in the first place divest himself of his robes of office.

For my own part I have thought that Mr. Lockyer has attached only too much importance to the little help I gave him when we conversed together in 1866.

BALFOUR STEWART

Mr. Mallet's Theory of Volcanic Energy

WILL you allow me to make a few remarks upon Mr. David Forbes's critique, which has but just now met my eye, though published in NATURE of February 6 last, upon my translation of Prof. Palmieri's "Incendio Vesuviano," and more especially upon that gentleman's animadversions upon my views as to "the true nature and origin of volcanic heat and energy," a brief and incomplete account of which I have given in the "introductory sketch" prefaced by me to that translation.

Mr. Forbes commences (p. 260) with an important error as to a matter of fact, by referring to "Mr. Mallet's Dynamical Theory of Volcanic Energy," as published in the Proceedings of the Royal Society for 1872.

My paper as above was read in abstract only in June last to the Royal Society, and being reserved for probable publication in the Phil. Trans., nothing but the most meagre and incomplete abstract has appeared in the Proceedings of the Royal Society.

I have given a somewhat fuller, but still most incomplete, account of it in my "introductory sketch" above referred to. My paper, and the full statement of my views, with the proofs which give them validity, have thus as yet never been published at all, nor even verbally stated publicly, the paper being still—after eight months, I regret to say—in the hands of the referees.

When published—as the paper in some form will no doubt be—Mr. Forbes will find that his objections—so far as I can gather anything tangible from their statement in NATURE—have been anticipated, and I believe completely answered, in my paper, along with others, better founded, because based on fact, which, as it appears to me, Mr. Forbes's objections are not.

Mr. Forbes uses "theory" and "hypothesis" as though quite the same (p. 260). My hypothesis is simply undeniable, being no more than that our globe is a terr-aqueous planet subject to secular refrigeration; and upon that hypothesis I have built up my theory, that the evolution of volcanic heat is a necessary result, upon acknowledged physical laws of such refrigeration; and that from such heat so evolved—as the *primum mobile*—come in train all the other recognised volcanic phenomena: chemical, as in the fusion and combination of the constituents of rocks into definite and indefinite compounds, decomposition of many solid, liquid, and gaseous bodies; mechanical in the elevation and throwing out at volcanic vents of all or any of these: water, air, and the chemical elements known to exist in the crust of our earth being the only conditions needed, in addition to heat, to account for all that we know as to volcanic phenomena, as now active in our planet.

I claim to have been the first to apply "measure, number, and weight" to volcanic theory; and when men of science generally shall have had access to my statements, I trust it will be admitted that I have not shrunk from rigidly testing by calculation the adequacy of the source I assign for volcanic heat, nor of the mechanism through which its subsequent effects are produced, as seen in volcanic phenomena. I am at the disadvantage that I cannot expect to cumber your pages by reference to those proofs—they are the less necessary here, however—inasmuch as my reviewer, though affecting to discredit the adequacy of the origin I assign for the "quantity of heat requisite to melt up such vast volumes of rock matter as are known to proceed from volcanoes," does not present a single argument against it, and, in fact, appears unwillingly to admit it. He however proceeds to object generally to my views by the following statements. They are so vague and inconsecutive that, as far as possible, I shall endeavour to condense them in his own words:—"Admitting that the conversion of the mechanical *force into heat* is sufficient to effect the melting part of the operation, there remains the greater difficulty of explaining the chemical and mineralogical features which characterise volcanic phenomena. For although mechanical *force* is admitted to be convertible into its equivalent heat, which in turn may 'cause' chemical action, still no such forces, alone or in combination, can transmute one

chemical element into another, or give rise to products having at all times a definite chemical and mineralogical constitution, out of the incongruous materials likely to be met with" in the crust of our globe.

"Our knowledge of the mineral characters of that crust does not admit the supposition that the substance of the rocks occurring in so many different parts of the world could in all or in any but solitary in tances when fused by mere heat afford products identical with those of known volcanoes.

"Volcanic products, no matter from what part of the world derived, are *identical in chemical or mineralogical constitution;* a result which indicates that they must be derived from some one common source, and not mere local accidents, as Mr. Mallet's *hypothesis* would require us to assume.

"For these and other reasons [none of which are suggested] it does not seem probable that this *hypothesis* will receive the adherence of either chemist, mineralogist, or geologist." The italics are mine.

Now I cannot help remarking that Mr. Forbes shows his very slender acquaintance with the vast subject he so jejunely disposes of, by the very language he employs; *force* cannot be converted into heat, equivalent or otherwise; *work, i.e.* pressure producing motion, is transformable into heat, and every other known form of energy. But passing this as well as the reiterated confusion between hypothesis and theory, I ask, is it a fact as stated, that volcanic products whencesoever derived, in time and in space over the whole globe, are *identical in chemical or mineralogical constitution.* I affirm unhesitatingly that it is not a fact, and I appeal for confirmation of my denial to every authoritative work on the subject, and to every well-informed mineralogical chemist.

Are the ancient basalts and trachytes identical with the modern ones, or with each other in different localities? Are the modern lavas of extinct or active volcanoes all over the world, or even at closely adjacent places, all identical? Are the highly liquid lavas of Otaheite, producing "Pele's hair," the same with the terrane lavas of Etna? Are the trachytes of Auvergne—the *Pierre de Volvie* so well known to French engineers, the same as those of Pozzuoli and of the Rhine? Are the products of the same volcanic basin ever identical—say the pumices and obsidians of Pouza, Ischia, and Lipari—with the innumerable varieties of lava of the same localities, and of the phlegræan fields and of Sicily? Are even the lavas of the same cone and during the same eruption always identical? Palmieri in the very report Mr. Forbes reviews, says (p. 105), "The *qualitative* analysis of the (Vesuvian) lavas always presents the same elements," with certain exceptions; "but with respect to *quantitative* analysis, two specimens of the same lava have their constituents in different proportion." Nay more, are even the crystallised minerals of definite chemical constitution, as segregated from the lava pastes or glasses, *identical* all over the world, or even for any one cone, or often for any one eruption? No, there is just that general similarity in the constitution of volcanic products that we should expect to result from the heating more or less, or the fusion together of the "incongruous" beds, siliceous, aluminous, magnesian, calcarious, mixed more or less with metallic oxides, or other compounds, and often with carbon, boron, and other elements all variously superposed or mixed, which constitute the known crust of our globe, along with most probably the materials of crystalline or other rocks below, the nature of which we can but guess at. There is just the same general similarity that is seen in the slags of all metallurgic furnaces, which fuse very similar materials to the volcano, but with a preponderance of some metal also. Are the slags of the same iron blast furnace *identical* even for any two tappings, though the working and charge is apparently the same? No, and for very obvious reasons. Are even the crystallised minerals that segregate from these slags, from the same furnace assumed, worked in the same way, always identical?

If Mr. Forbes will study Von Waltershausen's early and able work on Lavas, one scarcely known in this country, Zirkel's "Petrographie," Senft's "Kristallmischen Felsgemengtheile," and above all Blum's "Handbuch der Lithologie," amongst many others he will soon see how very baseless is the supposed fact on which he so confidently relies for his objection to my origin of volcanic heat.

But let us for a moment assume that it were a fact that all the volcanic products all over the world, and for all geologic periods, were "identical in chemical and mineralogical constitution," how would that form any relevant objections to the thermo-dynamic origination which I have assigned to volcanic *heat.* Or how would it help the old notion of a very thin crust and a universal molten ocean beneath, or the exploded one, that chemical action produces the heat, and the heat produces the chemical action.

Whatever be the origin assigned to the heat, it is within or below the heterogeneous solid crust of 10 to 40 miles thick, which we are acquainted with. More or less of that heterogeneous crust must be melted up, along with the material assumed everywhere the same of the molten ocean coming up through it from beneath.

It is proveable on merely hydrostatic grounds that the mass of such a molten ocean, if of materials such as are found in our earth's crust, could not be everywhere and in all latitudes identical in construction; but were it so, it must get contaminated and modified in passing through and melting the "incongruous" ducts of the crust bringing it to the surface.

On the other hand absolute identity everywhere in the volcanic products, did it exist, could prove nothing in favour of a universal ocean of ready-made lava as the source or origin of volcanic heat; but only that whatever might be the origin of that heat it was so circumstanced as to act on materials everywhere the same, and that these reached our surface without any action or mixture with the "incongruous" crust through which the molten matter came. There is just the same room for "local accident" in one case as in the other. A reviewer should at least be sure of his facts before he brings them into hostile array.

But again, what are the grounds for the assumption that according to my views, volcanic products of whatever sort are "the result of fusion by *mere* heat." I point out a true cosmical and thermo-dynamic origin for the heat itself, but under the actual conditions of volcanic foci and ducts, chemical actions must take place, as a consequence of the heat, and together vary the results of the reactions of all the materials present in fusibility, in aggregation, in their solid, liquid, or gaseous states, in their molecular conditions and in their chemical and crystallogenic combinations. This objection as little applies to anything I have written, as the irrelevant suggestion, that the transformation of energy "cannot transmute one chemical element into another."

I believe this disposes of whatever is tangible or worth remark in Mr. Forbes' strictures.

He ends, however, in a manner unusual I should think for an unbiassed reviewer, by uttering a sweeping prophecy, "for the reasons given [and above repeated], and others not brought forward, it is not probable that this hypothesis [*i.e.* my theory of volcanic energy] will receive the adherence of either chemist, mineralogist, or geologist." Prophecies, especially as to matters of science, are dangerous things, and the prophets would do well to rule by Swift's advice, as to utterances of another class, and never record one that can be falsified within a short time. It so happens that I am able already to explode Mr. Forbes's. Writing to me under date of Dec. 24 last (1872), Dr. Haughton, Professor of Geology of the University of Dublin, says—"I gave two lectures during the last term (Michaelmas) which were attended by many of the fellows, in which I developed your theory of volcanoes and gave you full credit. The whole system hangs well together and must make progress." Prof. Dana (in noticing this same book which Mr. Forbes so uncompromisingly condemns), as editor of the *American Journal of Science*, for the month of February, 1873, at p. 151, says: "His paper is the most important contribution to this department of geological dynamics that has ever been brought forward, and the work above-mentioned [*i.e.* Trans. of Palmieri] is by his share of it more than doubled in value." In a letter to me, Feb. 11, 1873, Prof. Dana says: "Your views throw great light on a hitherto mysterious department of geological science, and I have no doubt that they will speedily gain general acceptance."

I could add like expressions from some others. Now both Haughton and Dana are at once chemists, mineralogists, and geologists. They are both, also, sound physicists and mathematicians. So this prophecy is already falsified, and I am consoled with the notion that my theory of the nature and origin of volcanic energy may survive the unreserved condemnation to which Mr. Forbes has sentenced it, on the grounds that I have above examined, and without his ever having had access to the paper itself criticised by him. I might refute, too, some criticism on the book itself, which I cannot avoid calling unfair, and also not founded on fact. It is not true, for example, that the first forty-six pages of my introductory sketch "are but an abstract of my

previously published investigations into the theory of earthquakes" (p. 260). The reviewer may not have been able or not taken the trouble to distinguish the old from the new; but as a fact, the greater part of those forty-six pages is of matter never before published.

So also it is scarcely candid to object that "no reference is found to any of the Continental men of science who have done so much for terrestrial vulcanicity," which is contrary to the fact, for I have referred by name or by their labours to the few who have in any way advanced our knowledge as to the nature and origin of *volcanic heat*, without noticing that within that scope only was I by space obliged to confine myself, as stated in pp. 48, 49, 54, 76, &c.; the phenomena occurring at volcanic vents, which have chiefly engaged the attention of Continental and all other volcanic authors being avowedly outside my limits, and, I might add, but too often of secondary importance.

The nomenclature generally of my "Translation of Palmieri" is said to be objectionable, because such terms as sulphide of potass and terrochloride of ammonia are encountered. I have looked through the pages since without being able to discover these dreadful terms. However I am ready to take the reviewer's word that such a slip in proof correcting may be found in some place, and I humbly bow to such microscopic, profound, and valuable criticism, though, as stated, the conclusion is a good deal wider than its premises. ROBERT MALLET

Enmore, The Grove, S.W., March 5

Effect of Resistance in modifying Spectra

IN a review of M. Guillemin's work "The Forces of Nature" which appeared in last week's *Athenæum*, the following reference, by M. Guillemin, to the experiments of M. Mitscherlich is quoted: "Suivant ce physicien il arrive que la presence de certaines substances dans une flame a pour effet d'empecher de se produire les spectres des autres substances, d'enteindre leurs raies principales." The English editor adds that the effect "may probably be explained by the observations of Frankland and Lockyer."

In relation to this subject of the extinction of the bands of one metal by another, you will perhaps permit me to quote a paragraph from one of the lectures which I have recently had the honour of delivering in the United States. The arcs of thallium and silver had just been compared, and their similarity of colour pointed out. The power of prismatic analysis to show that, notwithstanding the apparent identity of colour, the arcs really belonged to two different metals, was then demonstrated. The metals were afterwards subjected *together* to the action of the Voltaic current, and it was shown that the band of thallium fell midway between the two bands of silver. Hence the similarity of colour. The lecture then proceeds thus:—

"But you observe here another interesting fact. The thallium band is at first far brighter than the silver bands; indeed the latter have wonderfully degenerated since the bit of thallium was put in. The reason of this is worth knowing. It is the *resistance* offered to the passage of the electric current from carbon to carbon that calls forth the heating power of the current. If the resistance were materially lessened, the heat would be materially lessened; and if *all* resistance were abolished there would be no heat at all. Now thallium is a much more fusible and vaporisable metal than silver, and its presence facilitates the passage of the current to such a degree as to render it almost incompetent to vaporise the more refractory silver. But the thallium is gradually consumed; its vapour diminishes, the resistance consequently rises, until finally the silver bands are rendered as brilliant as at first."

In the spectra of mixed substances derived from the electric spark the action here referred to must come frequently into play. If neither the fact, nor its proposed explanation, be new, I would thank you to commit this document to your waste-paper basket. JOHN TYNDALL

Royal Institution, March 1873

Perception in the Lower Animals

THE theory of taking olfactory notes by the way, as suggested by Mr. Wallace in explanation of the faculty possessed by animals of finding their way home, seems to meet with general acceptance amongst your correspondents; yet it totally fails to account for those instances in which the animal finds its way back by quite a different route to that by which it was taken away.

A good example is given by "F. R. G. S.," in the last number of NATURE; the anecdote of his riding-horse, by Mr. Darwin, also seems to illustrate this point. In an article on the "Consciousness of Dogs," in the *Quarterly Review*, of last October, the following remarkable instance, amongst others, is mentioned on indisputable authority. A hound "was sent by Charles Cobbe, Esq., from Newbridge, county Dublin, to Moynalty, county Meath, and thence, long afterwards, conveyed to Dublin. The hound broke loose in Dublin, and the same morning made his way back to his old kennel at Newbridge, thus completing the third side of a triangle by a road he had never travelled in his life."

Now as Mr. Wallace's theory does not explain these and similar instances, it clearly cannot be received as a solution of the question. Moreover, not only does the faculty exist in other animals not remarkable for their sense of smell, but we find it in cases where this sense has nothing to do with it. Take, for example, the direct homeward flight of the carrier pigeon. Under the same head may be brought the migrations of birds and fishes, and the habits of the turtle, as mentioned by Mr. Darwin.

The writer in the *Quarterly* suggests a sense of the magnetic currents of the earth—a sort of internal mariner's compass in fact. But it is difficult to see how this could have helped the dog to find its way from Dublin to Newbridge, for instance, unless it was also able to consult a map so as to ascertain the relative position of the two places.

It seems then that the problem still remains unsolved. Either we must extend almost indefinitely the range of smell and sight; or, we must suppose the existence of some peculiar sense of the nature of which we are ignorant, which enables its possessor to retain, as F. R. G. S. expresses it, "a constant perception of the bearing of its old home." J. T.

Bath, March 17

POSSESSION ISLES

AS the idea of occupying Possession Islands as a station for observing the Transit of Venus has been lately propounded, I have been requested to communicate to NATURE the results as to its climate, which we have obtained in this office from the logs of H.M.S. *Erebus* and *Terror*, which we are now re-discussing with a view to publication.

Possession Isles are in lat. 71° 56′ S., long. 171° 7′ E. H.M.S. *Erebus* and *Terror* were within lat. 70° to $72\frac{1}{2}$° S., and long. 170° to 175° E. from 10th to 17th January, 1841. During these eight days the mean height of the barometer was 29·143, mean temperature of the air 29°·7, and of the sea 30°·5; the wind was variable, but chiefly from S. and SSW., force 6; the weather was clear ten times, cloudy twenty times, overcast eighteen times, from forty-eight double sets of four-hourly observations, while snow was noted nine times, and squally weather ten times.

The ships were within the same area on 20th and 21st February, 1841; and, during these two days, the mean height of the barometer was 28·920 inches, mean temperature of the air 23°·5, of the sea 30°·1; wind WSW. to SE., force 9 to 5; the weather was cloudy and overcast.

In addition I am permitted to enclose a letter from Dr. Hooker, which he kindly sent me in reply to my inquiries as to his reminiscences of his visit to these inhospitable regions, and which he has allowed me to publish.

Meteorological Office ROBERT H. SCOTT

Letter from Dr. Hooker

Possession Island, or rather Possession *rock*, is in a very inaccessible position. The chance of landing a well-equipped party upon it when reached, and the prospect of its subsequent removal by ships, if landed on, is very small. In any case I feel little uncertainty as to what would be the fate of a party left there for the winter, and the prospect of their seeing the transit would be absolutely *nil*.

To reach it we "took the pack" January 3, 1841, and had not penetrated it till the 9th, aided at last by a furious gale. We then discovered South Victoria, and traced its coast from lat. $70\frac{3}{4}$° to lat. 78°, without finding a spot where it was possible to approach the shore. During the

twenty-two days that we spent off that continent, we never effected a landing but twice, and then, with the greatest difficulty, on two small volcanic islets, without a particle of vegetation on them, of which one was Possession Island (Jan. 13), *a mere rock*. The ship was hove to two miles off; with the greatest risk a landing was effected, on a beach of large loose stones and stranded masses of ice. It was no sooner done than the recall flag was hoisted in the ships, which were reached just as a terrific fog came on, followed by a gale of wind; ten minutes more and all hands in the boats would have been lost, for the currents ran like sluices between the land, islets, and icebergs. So much for Possession Island. (Read Ross's account of the landing, i. 188, and especially the paragraph at p. 190.)

Take a glance at the meteorological registers in Ross's voyage for the month of January 1841, which was passed between S. lat. 66° 32′ and 78°. The mean temperature was 29°·02, max. 41°·5, min. 19°·5. It snowed on sixteen days; overcast, squally and misty was the usual weather, blue sky was rarely seen over more than a quarter of the heavens for a very few hours of the day, and for many days not seen at all.

In March between lat. 77° and 69½°, the mean temperature was 24°·28, max. 34°, min. 13°. Sky as in January.

In the following year our vessel went to the same seas. We "took the pack" December 17, and after being all but wrecked, penetrated it after fifty-six days of great peril, and proceeded to 78° S., never once seeing land.

During that January within 66° 32′, and 67° 21′ the mean temperature was 30°·46, max. 40°·5, min. 24°. It snowed on seventeen days, and we hardly ever saw blue sky.

In February between lat. 67° 18′ and 78° 12′, the mean temperature was 26°·68, max. 35°, min. 16°·5, and it snowed on twenty days. Blue sky was seen only on thirteen days. In 1842 the weather was worse than ever. In that year we tried to get south in the meridian a little east of Cape Horn, but never got beyond lat. 71½°, and then not till March 6th, having left the Falklands on the 18th December. In January of that year (1842) we were between lat. 63° 58′, and 64° 44′. The mean temperature was 30°·9, max. 45°, min. 23°·5. It snowed on sixteen days—sky as before.

February—between lat. 61° 37′ and 66° 01′. The mean temperature was 30° 50, max. 35°·5, min. 27°·5. It snowed on twenty-four days out of the twenty-eight! Blue sky was seen only on seven days, and this on six days over one-eighth of the sky, and on the 7th over one-fourth.

With such a midsummer climate I leave you to guess the position of a party in lat. 72°, cooped up through a winter on a rock a few yards long, covered with snow.

During the third year's cruise to the southward, Captain Crozier never once went to his cot, and we passed day and night with our hearts at the top of our throats.

The fact is, there is no summer or clear weather to be had, except by the rarest chance. For days and days we worked by Dead Reckoning alone. Storm, wind, and snow, are the prevalent summer phenomena. Still some seasons are not so bad as others, and Weddell got to 74¼° in an open sea in the meridian where we barely reached 66°. (Signed) J. D. HOOKER

Royal Gardens, Kew, March 6

The following is the account of the landing alluded to by Dr. Hooker: —

"We found the shores of the mainland completely covered with ice projecting into the sea, and the heavy surf along its edge forbade any attempt to land upon it; a strong tide carried us rapidly along between this ice-bound coast and the islands amongst heavy masses of ice, so that our situation was, for some time most critical; for all the exertions our people could use were insufficient to stem the tide. But taking the advantage of a narrow opening that appeared in the ice, the boats were pushed through it, and we got into an eddy under the lee of the largest of the islands, and landed on a beach of large loose stones and stranded masses of ice. . . . . The island is composed entirely of igneous rocks, and only accessible on its western side. We saw not the smallest appearance of vegetation, but inconceivable myriads of penguins completely and densely covered the whole surface of the island, along the ledges of the precipices, and even to the summits of the hills, attacking us vigorously as we waded through their ranks, which, together with their loud coarse notes, and the insupportable stench from the deep bed of guano, which had been forming for ages, made us glad to get away again, after having loaded our boats with geological specimens and penguins. Owing to the heavy surf on the beach, we could not tell whether the water was ebbing or flowing; but there was a strong tide running to the south, between Possession Island and the mainland, and the *Terror* had some difficulty to avoid being carried by it against the land-ice. Future navigators should therefore be on their guard in approaching the coast at this place."

EARTHQUAKE WAVES

THE self-registering tide-gauges maintained by the United States Coast Survey at different points on the sea coast frequently exhibit, superimposed upon the tidal fluctuation, a succession of long waves, the origin of which is ascribed to distant earthquakes. In two notable instances, viz., the earthquake of Simoda in 1854, and that of Arica in 1868, the great ocean waves caused by the disturbance were distinctly registered in that way by the tide-gauges on the Pacific coast, and have been made use of for estimating the average depth along the lines of transmission. (See Coast Survey Reports for 1855, 1862, and 1869.)

Similar fluctuations were registered on the morning of November 17, 1872, shortly after local midnight, on the tide-gauge at North Haven, on the Fox Island, in Penobscot Bay, Maine. The fluctuations continued from midnight until nearly six o'clock in the morning, at somewhat irregular intervals of about seventeen minutes from crest to crest, with an average vertical range of nine inches, the greatest wave being at three o'clock, with a height of twenty inches.

No corresponding earthquake phenomena have come to the knowledge of the Coast Survey Office, and it is probable that if such was the case, the shock occurred somewhere under the Atlantic Ocean.

THE CHALLENGER EXPEDITION

H. M.S. *Challenger* cast off from the jetty at Portsmouth at 11.30 A.M. on December 21, with a low barometer. A strong south-westerly breeze was blowing, and the drum up; so that, especially in a season like the present, the prospect was not promising for the first few weeks of her voyage round the world.

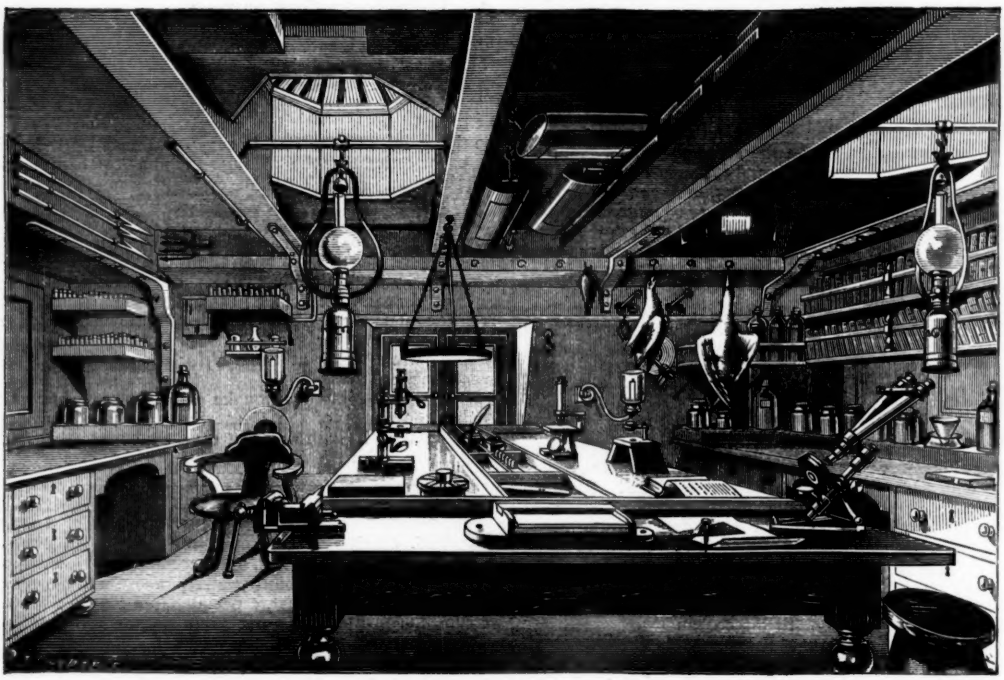
The result justified the drum, and for a week we were knocking about the mouth of the Channel, and the Bay of Biscay, making slow progress southwards. It was perhaps as well to get a good shaking at first. It showed at once where there was a screw loose, and gave a chance to tighten it up. A sharp cyclone which caught the ship on her way from Sheerness to Portsmouth had already tested pretty fully the stowing of the apparatus, and although the *Challenger* rolls considerably when she is put to it (over 35°), not a single instrument shifted, and not a glass was broken, either in the zoological workroom, or in the chemical laboratory. Just before we got to Lisbon the weather improved a little, and we got some soundings and took one or two trial hauls with the dredge.

After leaving Lisbon on January 12 the wind was again fresh, but between Lisbon and Gibraltar we made some important experiments, and found, among other things, that we could work easily and successfully with the common trawl down to 600 fathoms. I am now writing about 100 miles north of Madeira, and since leaving Gibraltar the weather, though at first breezy, has been on

the whole fine. We have taken several successful navigative sounds at great depths, and we have trawled successfully at 2,125 fathoms, and recovered many interesting animal forms, several of them new to science, and others of extreme rarity and beauty. Still we must regard our work up to the present time as only tentative. The weather has been against us. It is altogether a new experiment to dredge from so large a ship, and it seems to present some special difficulties, or at all events to require some management. The weight of the ship is so great that there can be no "give and take" between it and the dredge, such as we have in the case of a smaller vessel. If there is any way on, the impulse to the dredge is irresistible, and seems to tend to jerk it off the ground. This difficulty can no doubt be met, but the only way of meeting it appears to be by using a length of rope greatly in excess of the depth—and having weights. A single dredging operation may thus occupy a great length of time, but in compensation we have the greater size and efficiency of this dredge. The few trials which we have already made have been all in the direction of improvement, and I have little doubt that under Captain Nares' skilful management what little difficulty is still felt will shortly disappear.

As I hope to contribute to NATURE from time to time a series of articles giving the results of our voyage, it may be well to commence by giving a sketch of the general scope of our operations, and the means and appliances at our disposal.

The *Challenger* is a spare-decked corvette of 2,000 tons displacement. This particular build gives her an immense advantage for her present purposes, as she has all the accommodation of a frigate, with the handiness and draught of water of a corvette. Sixteen of the eighteen 68-pounders which form the armament of the *Challenger* have been removed, and the main-deck is almost entirely set aside



LABORATORY OF THE "CHALLENGER"

for the scientific work. The after-cabin is divided into two by a bulk-head, and the two little rooms thus formed—still gay with mirrors, and pictures, and new chintz, and bright with home faces—are allotted to Captain Nares and myself. The fore cabin, a handsome room, 30 ft. long by about 12 ft. wide, into which these private cabins open, the captain and I use as a sitting room, the port-end with its writing-table and work-table, and its book-cases packed with old home favourites, being appropriated to my use and that of my secretary Mr. Wild; while the captain has arrangements at the starboard end of the same kind. Two sets of cabins have been specially built on the after-part of the main-deck for this difficult part of the scientific work. On the port side a commodious zoological work-room is occupied by the naturalist of the civilian staff, while the chart room corresponds with it on the opposite side. Towards the middle of the main-deck on the port side there is a dark room and a working room, for the photographer, and on the starboard side Mr. Buchanan has his chemical and physical laboratory.

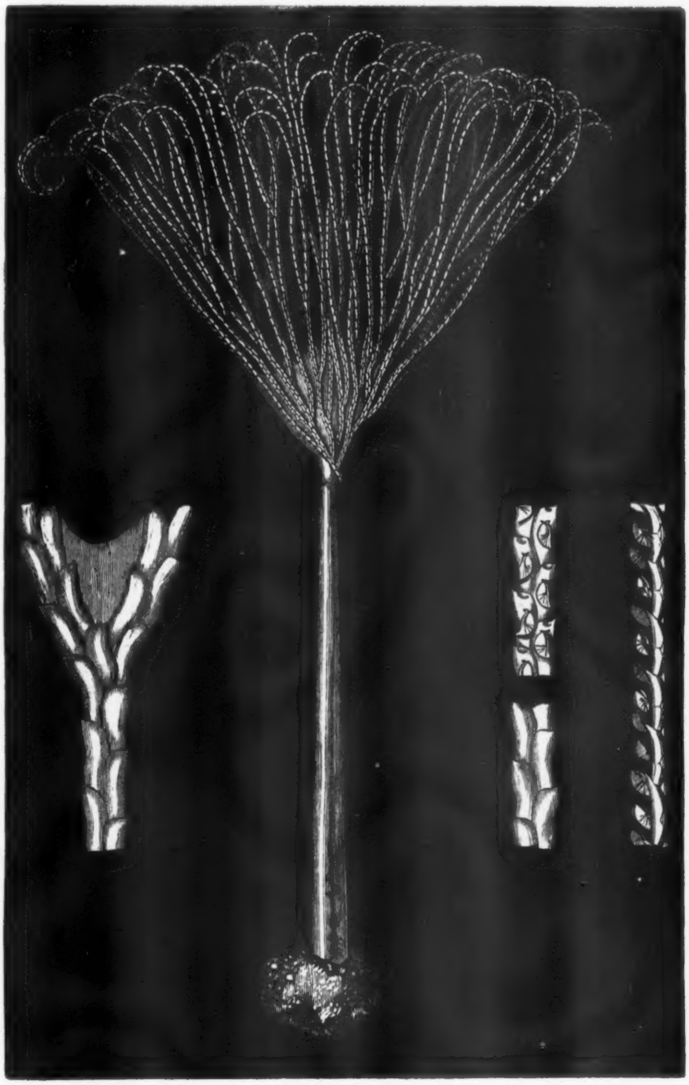
Nearly the whole of the fore-part of the main-deck is occupied by the dredging and sounding gear, Mr. Siemens's photometric and thermometric apparatus, and the more cumbrous of our machines, such as the hydraulic pump, the aquarium, and other very valuable articles, of which a detailed description will be given hereafter.

I feel justified in going even so far as to say that the arrangements for scientific work in the *Challenger* leave little or nothing to be wished for. Captain Nares and his officers not only do everything which care and skill can accomplish to further our objects, but, having naturally a certain advantage over the civilians in rough weather, they keep us up to the mark by the lively interest which they take in the success of our operations. There is a common mess in a large ward-room on the lower deck,

and the civilians have to heartily thank the naval men for the frank courtesy with which they have received them into their fellowship.

Dredging and sounding are carried on from the main-yard. A strong pennant is attached by a hook to the cap, and then by a tackle to the end of this yard. A compound arrangement of fifty-five of "Hodges' accumulators" is hung to the pennant, and beneath it a block through which the dredge rope passes. This arrangement appears to answer better than the old one of dredging from a derrick.

For the first two or three hauls in very deep water off the coast of Portugal, the dredge came up filled with the usual "Atlantic ooze," tenacious and uniform throughout, and the work of hours in sifting gave the very smallest possible result. We were extremely anxious to get some idea of the general character of the fauna, and particularly of the distribution of the higher groups; and after various suggestions for modifications of the dredge it was proposed to try the ordinary trawl. We had a compact trawl with a 15 ft. beam on board, and we sent it down off Cape St. Vincent at a depth of 600 fathoms. The experiment looked hazardous, but to our great satisfaction this trawl came up all right, and contained, with many of the larger invertebrata, several fishes. Two of these belonged to the genus *Macrourus*, and another of large size was unknown to us, approaching in many respects the genus *Mugil*. All the fishes were in a peculiar condition from the expansion of the air contained in their bodies. On their relief from the extreme pressure, their eyes especially had a singular appearance, protruding like great globes from their heads.



NARESIA CYATHUS

After this first attempt we tried the trawl several times at depths of 1,090, 1,525, and finally 2,125 fathoms, and always with success.

Several fishes, most of them allied to *Macrourus*, were added to the list. Several decomposed crustaceans, and among the lower crustaceans at 1,090 fathoms, a gigantic amphipod, of the family Hyperina, allied to *Phronima*. The eyes of this creature are very remarkable, extending in two great facettio lobes over the whole of the anterior part of the cephalo thorax, like the eyes of *Aeglina* among Trilobites. This crustacean, which is three and a half inches in length, makes a splendid drawing, and reminds one of the old Eurypterids, is in process of description at the hands of Dr. von Willemoes Suhm.

Mollusca are very scarce in deep water, and our catches have hitherto been chiefly confined to such things as the species of *Nucula, Leda, Verticordia*, &c., familiar through the deep dredgings of the *Porcupine*. Among the molluscoids a haul in 1,525 fathoms gave us a lovely thing, a bryozoan forming, out of branches closely resembling those of *Accromarchis neritina*, a graceful cup, the bases of the branches united by a transparent stem between two and three inches high, like the barrel of a quill, or the stem of a claret glass. This genus, which presents a general character totally different from anything hitherto known among recent Bryozoa, I mean to dedicate to Captain Nares, as an early recognition of the confidence and esteem which he has already fully gained from the scientific staff. *Naresia cyathus* certainly recalls, in a most singular way, the Cambrian *Dictyonema*, a form which I had, however, hitherto been inclined to refer to the Hydrozoa.

The Echinoderms have yielded some exceedingly interesting species to the trawl; among them several examples of the beautiful little urchins, of which one specimen was taken by Count Pourtales, in the Straits of Florida, and described by Alexander Agassiz under the name of *Salenia varispina*. It is undoubtedly a true *Salenia*, and to an advocate of the doctrine of the "continuity of the chalk," it is pleasant to see in the flesh this little beauty, which has hitherto been reckoned among the lost tribes.

Among the star-fishes two species of the genus *Hymenastes* have occurred, and the ophiurids are well represented chiefly by large examples of several species of the genus *Ophiomusium*.

All the hauls of the trawl, down to 2,125 fathoms, have yielded many specimens of a singular Holothurid, of which a description will shortly be published by Mr. Moseley. The animal is of a rich violet colour. Like *Psolus*, it has a distinct ambulating surface, with a central double line of water-feet. The body cavity is small, but the perisom is represented by an enormously thick layer of jelly, which rises on either side of the middle line of the back into a series of rounded lobes, each perforated for the passage of an ambulacral tube and corresponding therefore with an ambulacral foot. The upper pair of vessels of the trivium send out series of leaf-like sacs, richly loaded with pigment, which fringe on either side the ambulatory disc, and appear to be chiefly concerned in the function of respiration.

Sea-peas and Gorgoniæ have occurred frequently, always remarkable for their brilliant phosphorescence. Captain Maclear is giving special attention to this beautiful phenomenon. A *Mopsea*, which shone very brilliantly, gave a spectrum extending from the green well on into the red, while *Umbellularia* gave a very restricted spectrum sharply included between the lines *b* and D of this wonderfully rare sea-pea. We took with the trawl a very fine specimen, with a stem 3 ft. long, at a depth of 2,115 fathoms off Cape St. Vincent.

As usual in deep-sea work sponges preponderated, and the order has added several novelties, chiefly referable to the ventriculite group, the Hexactinellidæ.

Some fine new species of *Aphrocallistes* came up along the coast of Portugal, and off St. Vincent; with many spicules and more or less mutilated examples of Hyalonema, two or three species in fair condition of a species of *Euplectella*, with spicules which I cannot distinguish from those of *Euphalelles aspergillum*—the Venus flower-basket of the Philippines. The form of the two sponges is the same, but our own specimens are quite soft, the spicules not fused into a continuous siliceous network.

The physical and chemical observations will be fully detailed hereafter. The temperatures off the Coast of Portugal corresponded very closely with those taken in the *Porcupine* in 1870, and the *Shearwater* in 1871, below the first 100 fathoms through which at this season the temperature is nearly uniform. WYVILLE THOMSON

PROF. FLOWER'S HUNTERIAN LECTURES

LECTURES X. XI. XII.

THE fossil *Dasypodidæ*, or their existing allies, are found in America only. They may be divided into two classes, those closely resembling existing species, and those differing considerably from them. Most of the former have only been obtained in a fragmentary state; they have been studied by Lund; one was peculiar in having the teeth compressed from before backwards, instead of laterally. The latter class includes a well-known form, *Glyptodon*, of which there are several species, and perhaps more than one genus. An exhaustive monograph is in course of publication by Burmeister on the genus, and the material he has at his command at Buenos Ayres is very large. In 1839 Prof. Owen was among the first to describe it, he did so from the specimen now in the Museum of the College, giving it the name by which we know it. The Danish naturalist, Dr. Lund, at about the same time gave the name *Hoplophorus* to the remains of a very similar animal, and Burmeister considers that there is sufficient difference between the two type specimens of these authors, that of the latter having one less hind toe, to justify the generic name proposed by each being retained. In *Glyptodon* the carapace is proportionately thicker and stronger than in existing Armadillos; it is composed of distinct pieces in contact at their edges, but not anchylosed, except in aged individuals; it is never hinged transversely, so the animal could not roll itself up. A horny epidermis undoubtedly covered the shield, and hairs may have been present, as foramina are frequently found. The scutes vary in shape in different species, and are of different sizes in different parts of the same individual. The tail formed a thick cylindrical scute-covered column, which in some cases was armed with spines and swollen near the tips, like a giant's club. An approach to this peculiar shape is seen in the existing *Chlamydophorus*. All known species have eight molars above, and the same number below on each side of each jaw, in a long straight, nearly parallel series, running very far back. The teeth all grew from persistent pulps, and were therefore long and slightly arched, with two deep flutings on each side, whence the name. In structure they were much as in the Armadillos, and the presence of the grooves caused the central harder osteodentine to assume a somewhat gridiron shape, which was sometimes much elaborated. The front of the skull was much truncated, and a strong ossified septum was often present. Burmeister thinks that the animal possessed a trunk. The brain was proportionately very small, the olfactory lobes and cerebellum preponderating. Much of the skull was occupied by air cells and the molar roots. The descending zygomatic process was very large, to give origin to the masseter muscle; it is absent or nearly so in the Armadillos; it may differ in character from that of the Megatherium, probably arising from the maxillaries, as it was

perforated at its base by the infraorbital foramen. The vertebral column was most peculiar. Of the seven cervical vertebræ the first and sixth only were free, then came the "trivertebral bone" of Prof. Huxley, formed of the last cervical and first two dorsals; this was hinged ginglymoidly by the transverse processes on the next mass, which was composed of the rest of the dorsal vertebræ. The lumbar vertebræ were anchylosed together, but not to the last (the 13th) dorsal. The centra of the vertebræ were only represented by a thin bony plate which helped to form the tube for the spinal cord. Prof. Huxley thinks that the joint in the dorsal region was connected with the respiratory process, Burmeister considers that it enabled the animal to retract its head. The pelvis was much as in the Armadillo; the ilia were perpendicular. The symphysis pubis was slender, and is but infrequently preserved. The scapula possessed the characteristics of the class; the humerus had a supracondyloid foramen, and the radius and ulna were not anchylosed. The ungual phalanges were all hoof-like. As in the armadillos and seals, the 4th metacarpal bone articulated with the cuneiform as well as with the unciform, and the 5th with the latter only; the pollex was absent. There was no third trochanter to the femur; the astragalus was normal; and there were four or five toes to the hind foot.

Of the other extinct Edentata there are none closely allied to the Ant-eaters, Ant-bears, or Pangolins. In the Upper Miocene of Darmstadt an ungual phalanx has been found, which like those of Manis is longitudinally split; it has been named *Macrotherium* by Lartet. Teeth have been found since, much like those of Dasypus, so it could not have been a Manis. At Pikerme similar remains have been found, which have been named as parts of *Ancylotherium*. These animals must form a separate division of the Edentata.

The *Ungulata* are the next great group to be considered. In their teeth they all tend to the typical formula i. $\frac{3}{3}$ c. $\frac{1}{1}$ p. m. $\frac{4}{4}$ m. $\frac{3}{3} = 44$. The milk teeth are always functional and generally remain until the animal is nearly fully grown. The limbs are formed for simple support and progression, and there is no trace of clavicles. Except in the camels, the hoof encloses the ungual phalanx. Leaving Hyrax out of consideration it may be stated that the pollex and hallux are always absent. The class is found in all the world but the Australian region; they have not been found fossil in strata below the tertiaries, but in the oldest of them. They are absolutely divisible into two sub-classes from characters indicated by, but not entirely dependent on the structure of the feet. This division was indicated by Cuvier, but developed by Owen and H. N. Turner, Jun. Owen introduced the terms Perissodactylata and Artiodactylata by which these sub-classes are best known. In the *Perissodactylata* the middle toe is symmetrical, and there is typically one toe on each side of that, except in the Tapir and an early fossil Rhinoceros, *Acerotherium*. The astragalus has a single large anterior facet, entirely or mainly for articulation with the naviculare. There is a third trochanter to the femur, and there are never less than twenty dorso-lumbar vertebræ. The nasal bones expand posteriorly, and there is an osseous alisphenoid canal, as pointed out by Mr. Turner. In the *Artiodactylata* the axis of each of the feet is between the middle and fourth toe, and there is one toe outside each of these, but in the Peccary one is absent in the hind foot. The astragalus supports both the navicular and cuboid bones on nearly equal-sized facets. There is no third trochanter to the femur, and there are always nineteen dorso-lumbar vertebræ. There is no alisphenoid canal; the palate is completed by bone opposite the posterior molars. All these characters, especially when taken in connection with the teeth, make it easy, from a few fragments of the skeleton, to identify the sub-class to which fossil members of the class belong. In the Perissodactylata the persistent premolars and the molars are very much alike, being all double, but in the Artiodactylata the premolars are single, and therefore they do not form a uniform series with the molars; there is a third lobe to the last lower molar, except in an antelope, *Neotragus Saltiana*, as lately proved by Sir V. Brooke.

The earliest of the Perissodactylata are the *Lophiodontidæ*, from the lower Eocene. They are rather more generalised than the existing forms, as would be expected on the evolution hypothesis. The premolars are simpler than those behind. *Coryphodon*, the oldest, is known by its teeth, of which it possessed the typical forty-four, and a femur with a third trochanter. Its molars had two ridges with conical apices, whence Owen gave it its name, from a specimen dredged off Harwich. The feet are not known unfortunately.

Lophiodon itself is only known by its teeth and fragments of the skeleton; the upper part of the skull has not yet been found. It is a genus of the early and middle Eocene only. Its dental formula was i. $\frac{3}{3}$ c. $\frac{1}{1}$ p.m. $\frac{3}{3}$ m. $\frac{3}{3}$. The molars are representatives of the type which runs through the whole class. In the upper jaw each tooth presented an outer wall, which developed into two well-formed cusps; running back from this were two transverse ridges, an anterior and a posterior, as they are termed. The anterior transverse ridge springs from in front of the anterior cusp, the posterior from in front of the one behind; these ridges, each by their curving backwards, enclose a space named the anterior and posterior sinuses. The posterior transverse ridge is absent in the premolars. The lower molars are simpler. The premolars were reduced in a manner which characterised the genus, the hinder part being cut away. Lophiodon is mainly found in the lacustrine deposits of the south of France; there were several species, varying in size from a full-grown Indian rhinoceros to that of a hare. Leidy, from the deposits of Nebraska, found a tooth exactly resembling those of this genus; he has named it *L. occidentalis*, but acknowledges the insufficiency of the evidence on which it is founded.

The next animals to be considered were about the size of the hare. *Pachynolophus* differed from Lophiodon in having seven pre-molars and molars in the upper jaw instead of six; the number in the lower jaw is not quite so certain, some having apparently six and others seven. The ridges of the teeth were less considerable and more broken into tubercles. In the London clay, near Herne Bay, a skull was found, named by Prof. Owen *Hyracotherium*. Mr. H. N. Turner was the first to point out that this animal was one of the Perissodactylata, and not as Prof. Owen at first supposed, allied to the Suidæ. The teeth very closely resembled those of Pachynolophus, each transverse ridge developing into a median smaller tubercle and a posterior larger one; the pre-molars were also considerably smaller than those behind. From the resemblance of the teeth it is evident that the French genus Pachynolophus and the English Hyracotherium must be considered to be one. *Pliolophus* is the name of a genus given by Owen to a specimen obtained from the London clay off Harwich, together with a humerus, femur, and three metatarsals. The forty-four teeth were present. There is no reason to suppose that this genus is different from Hyracotherium, the shape of the teeth and the size being identical; Prof. Owen himself states the possibility of their identity. The fact of there having been three metatarsals or metacarpals as they may be, found together, is a collateral one in favour of the animal having been Perissodactylate. From the above remarks it is evident that the names Pachynolophus and Pliolophus must be sunk in favour of Hyracotherium, as must also *Lophiotherium*, a name given by Gervais to an Upper Eocene specimen, known only by the mandible.

At the same time lived another small animal, *Miolophus*, known only from a fragment now at York. It differs from most ungulates in having only a single inner cusp to the molars, so causing it to resemble a typical pre-molar. Another form, *Microchœrus erinaceus*, is very aberrant, and its position is doubtful.

TESTIMONIAL TO DR. BENCE JONES

WE regret very much to hear that Dr. Bence Jones has been compelled on account of his health to resign the office of Secretary to the Royal Institution, a post which he has filled for so many years with equal honour to himself and advantage to the Institution.

His conviction of the value of original research, and of the special vocation of the Royal Institution to continue diligent in promoting it, was with him an unceasing stimulus to exertion. His attention to every detail left nothing neglected in the performance of his duties. His own scientific attainments have been of signal effect in maintaining respect for the Institution, and in procuring the co-operation of eminent men in the laboratories and lecture theatre. His love of the place and its memories has been shown by the pains he took to collect its early annals; including in this work an account of the discoveries of Young and Davy, and by his becoming the historian of Faraday.

The services of Dr. Bence Jones have been given under the pressure of important professional engagements, and latterly under the additional difficulties of failing health; and until now, when he has been reluctantly compelled to resign, he has never relaxed in the active prosecution of his honourable task.

We trust with the managers, however, that the aid of Dr. Bence Jones may not be altogether lost to the Institution; but that he will still afford to it the benefit of his counsels and experience. It is hoped that he may in future occupy a seat at the Board of Management; and further, that he will remain associated with the Institution by doing it the favour of accepting the position of Honorary Assistant-Secretary.

It has very naturally been proposed to present Dr. Bence Jones with a testimonial to be raised by subscription, and we feel confident that to so worthy a purpose there will be no lack of willing contributors. Individual subscriptions are limited to 3*l*. 3*s*. as a maximum.

It has been ascertained that the form of testimonial most agreeable to Dr. Bence Jones would be a bust of himself to be placed in the Royal Institution. Subscriptions to this testimonial may be paid either at the Royal Institution, or to "The Dr. Bence Jones Testimonial Account," at Messrs. Drummonds, the bankers, Charing Cross, who are authorised to receive the same.

CAPTAIN M. F. MAURY

MATTHEW FONTAINE MAURY, whose death, on Feb. 1, we recently recorded, was of French descent, and was born in Spottsylvania County, Virginia, Jan. 24, 1806. While still a child, his parents, who were in moderate circumstances, removed to Tennessee, where young Maury was sent to school. In 1825, when nineteen years old, he entered the service of the United States as midshipman, circumnavigating the globe in the *Vincennes*, during a cruise of four years. During this cruise Maury began his well-known "Treatise on Navigation," which was finished some years afterwards, and was for a long time used as a text-book in the U.S. navy. In 1836 he was made lieutenant and was gazetted astronomer to an exploring expedition.

In 1839, while travelling on professional duty, Lieut. Maury met with an accident which resulted in permanent lameness and unfitted him for active service afloat. What appeared then as a great misfortune to the lieutenant resulted indirectly in an increase of his fame, and in the performance of services of high value to science and humanity. The lame lieutenant was placed in charge of the Dépot of Charts and Instruments, out of which have grown the Naval Observatory and the Hydrographic Office of the United States. He laboured assiduously from the first day of his appointment to organise this dépot more efficiently than formerly. How completely he succeeded is well known.

While sailing around the globe in the *Vincennes*, Maury made many observations as to the winds and currents. These he continued in his subsequent cruises. When he became superintendent of the Hydrographic Office he determined to do something towards elucidating the intricate subject of ocean meteorology. The beginnings of this great undertaking were small. Maury obtained at first copies of such log-books as he or his friends could command. He noted the direction of the wind, the currents, &c., on the maps which he had prepared. As the information came in, districts were filled up and places pointed out for investigation. In 1842 the system had taken such consistency in his own mind that the lieutenant communicated to the U.S. Naval Bureau of Ordnance and Hydrography a plan for supplying model log-books to the commanders of vessels in the naval and merchant marine service. These log-books are so arranged that a systematic series of observations might be registered. The plan succeeded so well that in eight or nine years he had thus collected a sufficient number of logs to make 200 manuscript volumes averaging each about 2,500 days' observations each. These materials were digested by a board of officers appointed for that purpose, and the more immediate result of their labours was to show the necessity for combined action on the part of the maritime nations in regard to ocean meteorology.

In order that his labours might lead to some practical result, Maury wrought zealously to bring about a meeting of meteorologists belonging to all maritime nations; this led to the conference which met at Brussels in 1853, at which England, France, Russia, Portugal, Belgium, Holland, Denmark, Norway and Sweden were represented, and which produced the greatest benefit to navigation, as well as indirectly to meteorology. One of its most eminent and practical results was the establishment in London of the Meteorological Department of the Board of Trade. It recommended a model log-book for all vessels, in which a brief and uniform register of the principal meteorological phenomena are entered. The British Admiralty, the Royal Society, and the British Association entered heartily into Maury's plans, and aided him in every possible way; though we are ashamed to say that England is almost the only civilised country in the world that did not confer on this great benefactor of humanity some mark of honour: other countries loaded him with well-deserved tributes of admiration and gratitude for his services.

At the outbreak of the American civil war in 1861 Maury threw in his lot with the South, and did much to strengthen its maritime defences and enable it to hold out for so long as it did. He afterwards retired to England, where he lived for many years, and where he was presented with a handsome testimonial raised by subscription, he having lost nearly his all through his attachment to the unfortunate South. Having offered his services to the ill-fated Maximilian, of Mexico, the latter appointed him Imperial Commissioner of Emigration; and after the fall of that short-lived empire, Maury returned to the United States, taking up his residence in Virginia, where he lived until his death, on February 1 last. During his later years he devoted his time and efforts to urging his fellow-citizens of the south to leave politics alone and

apply themselves energetically to the development of the agricultural resources of their country.

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At the same time lived another small animal, *Miolophus*, known only from a fragment now at York. It differs from most ungulates in having only a single inner cusp to the molars, so causing it to resemble a typical pre-molar. Another form, *Microchærus erinaceus*, is very aberrant, and its position is doubtful.

TESTIMONIAL TO DR. BENCE JONES

WE regret very much to hear that Dr. Bence Jones has been compelled on account of his health to resign the office of Secretary to the Royal Institution, a post which he has filled for so many years with equal honour to himself and advantage to the Institution.

His conviction of the value of original research, and of the special vocation of the Royal Institution to continue diligent in promoting it, was with him an unceasing stimulus to exertion. His attention to every detail left nothing neglected in the performance of his duties. His own scientific attainments have been of signal effect in maintaining respect for the Institution, and in procuring the co-operation of eminent men in the laboratories and lecture theatre. His love of the place and its memories has been shown by the pains he took to collect its early annals; including in this work an account of the discoveries of Young and Davy, and by his becoming the historian of Faraday.

The services of Dr. Bence Jones have been given under the pressure of important professional engagements, and latterly under the additional difficulties of failing health; and until now, when he has been reluctantly compelled to resign, he has never relaxed in the active prosecution of his honourable task.

We trust with the managers, however, that the aid of Dr. Bence Jones may not be altogether lost to the Institution; but that he will still afford to it the benefit of his counsels and experience. It is hoped that he may in future occupy a seat at the Board of Management; and further, that he will remain associated with the Institution by doing it the favour of accepting the position of Honorary Assistant-Secretary.

It has very naturally been proposed to present Dr. Bence Jones with a testimonial to be raised by subscription, and we feel confident that to so worthy a purpose there will be no lack of willing contributors. Individual subscriptions are limited to 3*l*. 3*s*. as a maximum.

It has been ascertained that the form of testimonial most agreeable to Dr. Bence Jones would be a bust of himself to be placed in the Royal Institution. Subscriptions to this testimonial may be paid either at the Royal Institution, or to "The Dr. Bence Jones Testimonial Account," at Messrs. Drummonds, the bankers, Charing Cross, who are authorised to receive the same.

CAPTAIN M. F. MAURY

MATTHEW FONTAINE MAURY, whose death, on Feb. 1, we recently recorded, was of French descent, and was born in Spottsylvania County, Virginia, Jan. 24, 1806. While still a child, his parents, who were in moderate circumstances, removed to Tennessee, where young Maury was sent to school. In 1825, when nineteen years old, he entered the service of the United States as midshipman, circumnavigating the globe in the *Vincennes*, during a cruise of four years. During this cruise Maury began his well-known "Treatise on Navigation," which was finished some years afterwards, and was for a long time used as a text-book in the U.S. navy. In 1836 he was made lieutenant and was gazetted astronomer to an exploring expedition.

In 1839, while travelling on professional duty, Lieut. Maury met with an accident which resulted in permanent lameness and unfitted him for active service afloat. What appeared then as a great misfortune to the lieutenant resulted indirectly in an increase of his fame, and in the performance of services of high value to science and humanity. The lame lieutenant was placed in charge of the Dépot of Charts and Instruments, out of which have grown the Naval Observatory and the Hydrographic Office of the United States. He laboured assiduously from the first day of his appointment to organise this dépot more efficiently than formerly. How completely he succeeded is well known.

While sailing around the globe in the *Vincennes*, Maury made many observations as to the winds and currents. These he continued in his subsequent cruises. When he became superintendent of the Hydrographic Office he determined to do something towards elucidating the intricate subject of ocean meteorology. The beginnings of this great undertaking were small. Maury obtained at first copies of such log-books as he or his friends could command. He noted the direction of the wind, the currents, &c., on the maps which he had prepared. As the information came in, districts were filled up and places pointed out for investigation. In 1842 the system had taken such consistency in his own mind that the lieutenant communicated to the U.S. Naval Bureau of Ordnance and Hydrography a plan for supplying model log-books to the commanders of vessels in the naval and merchant marine service. These log-books are so arranged that a systematic series of observations might be registered. The plan succeeded so well that in eight or nine years he had thus collected a sufficient number of logs to make 200 manuscript volumes averaging each about 2,500 days' observations each. These materials were digested by a board of officers appointed for that purpose, and the more immediate result of their labours was to show the necessity for combined action on the part of the maritime nations in regard to ocean meteorology.

In order that his labours might lead to some practical result, Maury wrought zealously to bring about a meeting of meteorologists belonging to all maritime nations; this led to the conference which met at Brussels in 1853, at which England, France, Russia, Portugal, Belgium, Holland, Denmark, Norway and Sweden were represented, and which produced the greatest benefit to navigation, as well as indirectly to meteorology. One of its most eminent and practical results was the establishment in London of the Meteorological Department of the Board of Trade. It recommended a model log-book for all vessels, in which a brief and uniform register of the principal meteorological phenomena are entered. The British Admiralty, the Royal Society, and the British Association entered heartily into Maury's plans, and aided him in every possible way; though we are ashamed to say that England is almost the only civilised country in the world that did not confer on this great benefactor of humanity some mark of honour: other countries loaded him with well-deserved tributes of admiration and gratitude for his services.

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in Germany; and the latter has published the result of his investigations in the "Monatsberichte" of the Berlin Academy. Both these acute observers are at issue with previous writers who maintained that the flowers of cereals, and especially of wheat, were self-fertilised in the unopened flowers, and consequently that the process could not be influenced by the wind. Hildebrand asserts, on the other hand, that impregnation takes place while the flower is open, and while the stigma is in a condition for the access of foreign pollen, that is, from other flowers. The opening of the flower of wheat, however, is completed in such a very short space of time that in a wheat-field there is probably never more than one in 400 of the flowers open at the same time. The contrivances by which in this case, as well as in other grasses, cross-fertilisation is at least rendered possible, are described in detail in the paper. In barley, on the other hand, the majority of the flowers never open, and self-fertilisation is the only condition possible. Delpino states, however, that there are in an ear of barley a very small number of flowers, differently constructed from the rest, in which cross-fertilisation is possible. In the oat the process is stated to vary according to the weather; in fine warm weather the flowers open freely, and cross-fertilisation is favoured; in cold wet weather they remain closed, and self-fertilisation is inevitable In rye, fertilisation from the pollen of other flowers is provided for. The agent in the dissemination of the pollen is scarcely ever insects, almost invariably the wind, to which end both stigma and pollen-grains are specially adapted.

WE regret to say that last week we were led to state that Mr. Cleminshaw, recently elected to the Burdett Coutts Scholarship, received his scientific training in the Applied Science department of that College. We are requested to say that this is incorrect, as Mr. Cleminshaw was only a few months at King's College, where he did not study geology at all, while he was for several years at Rugby, to which school really belongs the credit of having trained so successful a student: moreover, Mr. Cleminshaw is, we believe, the third Burdett Coutts' scholar from Rugby within a few years. It is a pity that any school should require to make haste to claim possession of an honour which is not justly its due.

A LIVING specimen of the extremely rare Liberian Hippopotamus (*Chœropsis liberiensis*), from Scarcies River, just north of Sierra Leone, arrived at Liverpool last week, but it unfortunately died on Friday, almost as soon as it reached its destination, Dublin. This second true hippopotamus was first described in 1844 by Dr. Morton, of Philadelphia, in the Journal of the Academy of Natural Sciences of that city. Prof. Leidy, in 1850, showed that its peculiarities rendered its differences from *Hippopotamus* more than specific, and in 1852 gave it the generic name by which it is now known. The full-sized animal is said to be no larger than a heifer, and the specimen under consideration, which was at least seven weeks old, weighed only 23 lbs., whereas the one born in London last November weighed just upon 100 lbs. shortly after birth. But the chief peculiarities of the genus *Chœropsis* are found in the teeth, as there are only two lower incisors instead of four, and the anterior premolars remain functional throughout the life of the animal, instead of being lost as is the case in *Hippopotamus*. In addition to these points in which *Chœropsis* is peculiar, it may be mentioned that the top of the head is convex instead of concave, the central upper incisors are slightly smaller than the outer, instead of larger, and the premaxillary bones are less developed than in Hippopotamus, from a young one of which, as M. A. Milne-Edwards remarks, it would be difficult to distinguish it externally.

FROM a memorandum affixed to the last part of Messrs. Sharpe and Dresser's "Birds of Europe," we learn that the latter gentleman has removed his entire collection of birds to No. 6, Tenterden Street, Hanover Square, where the work will in future be published, and which will henceforth be the head-quarters of the British Ornithologists' Union, as Messrs. Salvin and Godman have likewise removed their valuable collection of American birds to the same quarters, and Lord Lilford as well as Capt. Shelley have done the same with theirs.

A CHROME mine has been discovered at Chanli, in the province of Larissa, Turkey in Europe. It is said to be rich.

ON January 31, four shocks of earthquake were felt at Kara Hissar (query which) in Asia Minor. The shocks, of which the first two were rather violent, were repeated at intervals of twenty minutes. In the next afternoon, February 1, eleven more shocks were felt, which, though very sharp, only brought down three old abandoned houses. On Saturday, February 17, slight shocks of earthquake were felt in the island of Mitylene; the first at 1 A.M., and the second at 5.20 P.M. The oscillation was from west to east.—An earthquake that occurred on Dec. 29 last, at night, nearly destroyed the large town of San Vicente, in San Salvador, Central America. The upper half of the parish church fell, and many lives were lost.—At Bimlipatam, in India, a shock of earthquake was felt on January 25.—An earthquake is reported to have been felt at Peshawar, on Feb. 12. Its shock lasted for one minute.

ACCORDING to the *Times of India*, a phenomenon occurred in several parts of the province of Kattywar, on Feb. 12, which gladdened the eyes of the English residents, and excited astonishment in the natives. On that day the ground was whitened by a hailstorm, a phenomenon unprecedented in the experience of the natives, hundreds of whom are reported to have set about gathering the hailstones as they fell, in order to turn them into "the best of confectionaries."

THE collector of South Canara has brought to the notice of Government that the young shoots of the bamboo contain a bitter principle, which might be useful as a febrifuge.

THE Council of the Society of Arts will proceed to consider the award of the Albert medal early in May next. This medal was instituted to reward "distinguished merit in promoting arts, manufactures, or commerce," and was awarded last year to Mr. Henry Bessemer. The members of the Society are invited to forward to the Secretary, on or before April 12, the names of such men of high distinction as they may think worthy of this honour.

THE late Mr. George A. Clark, thread manufacturer, of Paisley, has left 20,000*l*. to Glasgow University for the establishment of four bursaries to be held by the successful competitors for four years, and to be so arranged that one bursary will be competed for each year. We hope the interests of science will not be forgotten in this matter.

THE additions to the Zoological Society's Gardens during the last week comprise a Condor Vulture (*Sarcorhamphus gryphus*) from S. America, presented by Lieut. L. C. Strachey, R.N.; a Lapland Bunting (*Centrophanes lapponica*), presented by Mr. F. Bond; a Mantell's Apteryx (*Apteryx mantelli*); an Amherst Pheasant (*Thaumelia amherstiæ*) from China, and a Tamandua Ant-eater (*Tamandua tetradactyla*) from S. America, deposited; two Swainson's Lorikeets (*Trichoglossus novæ-hollandiæ*) from Australia; two Red-eared Bulbuls (*Pycnonotus jocosus*) from India; two Cardinal Grosbeaks (*Cardinalis virginianus*) from N. America; a Long-tailed Glossy Starling (*Lamprotornis æneus*) from W. Africa; a Pantherine Toad (*Bufo pantherinus*) from Tunis; some Natterjack Toads (*Bufo calamita*) from Africa; a Bearded Lizard (*Amphibolurus barbatus*) from Australia; five Banded Sea Horses (*Hippocampus ramulosus*) from France, and some Anemonies, all purchased.

THE BIRTH OF CHEMISTRY
VIII.

General Character of Alchemy and the Alchemists.—The Pretiosa Margarita Novella.*—An Alchemistical Allegory.—Alchemical Symbols.—Paracelsus.—Libavius.*

WHAT manner of men were the Alchemists? How did they preserve, cultivate, and transmit the wonderful delusions of their creed? We have endeavoured in a former article to show that the idea of transmutation arose from the old Greek idea of the conversion of one element into another; and the belief in the possibility of transmutation once admitted, the pursuit of the alchemist would naturally follow in a mystical and credulous age. As to the men themselves, their character was twofold; for there was your alchemist proper, your true enthusiast, your ardent, persevering worker, who believed heart and soul that gold could be made, and that by long search or close study of the works of his predecessors, he could find the Philosopher's stone; and there was your knavish alchemist, a man who had wits enough to perceive that the search was futile, and impudence enough to dupe more credulous people than himself, and wheedle their fortunes out of them on pretence of returning it tenfold in the shape of a recipe for converting lead into gold. These last we may dismiss at once. They abounded during the Middle Ages, and found easy dupes, whom they deceived by the most shallow tricks, as by placing a piece of gold in the crucible of transmutation together with volatile substances, and after many processes and much heating, they would show the little button of metal which had all along been present.

Of the true alchemist we have many pictures. The alchemist, the astrologer, the mystic, the wizard, were men of the same stamp. They often practised the same arts side by side. The same habit and attitude of thought belonged to one and to all, and became all equally well. Take the dreamy, maudlin, semi-maniacal Althotas, who has been described so well by Dumas:—"An old man, with grey eyes, a hooked nose, and trembling but busy hands. He was half-buried in a great chair, and turned with his right hand the leaves of a parchment manuscript." Note also his intense abstraction, his forgetfulness of the hour, the day, the year, the age, the country; his absolute and intense selfishness, and absorption, the concentration of the whole powers of his soul upon his one object. Or let us look at Victor Hugo's Archidiacre de St. Josas, in his search for the unseen, the unknown, and the altogether uncanny; the bitterness of his soul, his passionate musings, his conjurations and invocations in an unknown tongue; his own self, that wonderful mixture of theologian, scholar, mystic, perhaps not much unlike the divine S. Thomas Aquinas himself. Listen to his musings: "Yes, so Manon said, and Zoroaster taught:—the sun is born of fire, the moon of the sun; fire is the soul of the universe; its elementary particles are diffused and in constant flow throughout the world, by an infinite number of channels. At the points where these currents cross each other in the heavens they produce light, at their points of intersection they produce gold. Light!—gold! the same thing; fire in its concrete state. . . . . What! this light that bathes my hand is gold? The first the particles dilated according to a certain law, the second the same particles condensed according to another law! . . . For some time, said he, with a bitter smile, I have failed in all my experiments; one idea possesses me, and scorches my brain like a seal of fire. I have not so much as been able to discover the secret of Cassiodorus, whose lamp burned without wick or oil—a thing simple enough in itself." If we peep into Dom Claude's cell, we are introduced to a typical alchemist's laboratory—a gloomy, dimly-lighted place, full of strange vessels, and furnaces, and melting pots, spheres, and portions of skeletons hanging from the ceiling; the floor littered with stone bottles, pans, charcoal, aludels, and alembics, great parchment books covered with hieroglyphics; the bellows with its motto *Spira, Spera;* the hour-glass, the astrolabe, and over all cobwebs, and dust, and ashes. The walls covered with various aphorisms of the brotherhood; legends and memorials in many tongues; passages from the Smaragdine Table of Hermes Trismegistus; and looming out from all in great capitals, ἈΝΆΓΚΗ. Yet once again, look at Faust, as depicted by Rembrandt, or Teniers, unknown alchemist, if you wish for an alchemical interior.

But the hard-working and enthusiastic alchemist did not always follow the ideal of the novelist and artist; he often degenerated into a "dirty, soaking fellow," who lost what little learning he ever had by concentrating his mind on the one dominant topic, until it excluded every other idea and aspiration; then the pursuit became all absorbing, and the disciple of the art a mere drivelling monomaniac.

We will now look at one of the books which were cherished by the alchemists. Here is a little vellum-covered *Aldus*: date 1546. Paracelsus had been dead five years, and Cornelius

FIG. 14.—Allegorical representation of transmutation.

Agrippa, twelve years; Dr. Dee and Oswald Crollius were flourishing; Van Helmont and a host of known alchemists were unborn. Our little volume, full of quaint musings of a bygone age, has outlived them all, and yet it never drank of the *elixir vitæ*, although it pretended to teach others how to make it, and the philosopher's stone into the bargain. *Pretiosa Margarita Novella de Thesauro, ac pretiosissima Philosophorum Lapide* is the title; published with the sanction of Paul III. Pontifex Maximus, whose successor, be it remembered, established the *Index Expurgatorius*, and might possibly have prohibited the Precious Pearl of alchemy. The title-page goes on to tell us that it contains the methods of the "divine art," as given by Arnaldus de Villâ Novâ, Raymond Lulli, Albertus Magnus, Michael Scotus, and others, now first collected together by Janus Lacinius. The vellum cover is well thumbed, and in one place worn through, perhaps by contact with a hot iron on an alchemist's furnace-table, or by much use. There are no MS. notes, but on the title-page is the autograph of Sir C. Koby, or Hoby, and a favourite maxim, the first word of which alone—*Fato*—is legible. The date of the writing is perhaps 1580-90. Some initial letters of the text have been plainly illuminated in

FIG. 15.—Allegorical representation of transmutation.

red, by a loving hand; they were copied from a bible printed at Lyons in 1326.

As to the contents we have firstly an opening address by Janus Lacinius, then certain definitions of form, matter, element, colour, &c. Next, symbolic representations of the generation of the metals, and after this a woodcut representing the transmuta-

tion of the elements according to the dogmas of Aristotle.* After this we find the whole course of transmutation set forth pictorially and allegorically, as under. A king (see Fig. 14) crowned with a diadem, sits on high, holding a sceptre in his hand. His son, together with his five servants, beseech him on bended knees, to divide his kingdom between them. To this the king answers nothing. Whereupon the son at the instigation of the servants, kills the king and collects his blood. He then digs a pit into which he places the dead body, but at the same time falls in himself, and is prevented from getting out by some external agency. Then the bodies of both father and son putrefy in the pit. Afterwards their bones are removed, and divided into nine parts, and an angel is sent to collect them. The servants now pray that the king may be restored to them, and an angel vivifies the bones. Then the king rises from his tomb, having become all spirit, altogether heavenly and powerful to make his servants kings. Finally he gives them each a golden crown, and makes them kings (Fig. 15).

It is difficult to follow this from beginning to end, but there can be no doubt that the king signifies gold, his son, mercury, and his five servants the five remaining metals then known, viz. iron, copper, lead, tin, and silver. They pray to have the kingdom divided amongst them, that is to be converted into gold; the son kills the father, viz. the mercury forms an amalgam with gold. The other operations allude to various solutions, ignitions, and other chemical processes. The pit is a furnace; putrefaction means reaction or mutual alteration of parts. At last the philosopher's stone is found, the gold, after these varied changes becomes able to transmute the other metals into its own substance. At the end some rugged hexameters and pentameters warn the fraudulent, the avaricious, and the sacrilegious man that he is not to put his hands to the work, but to leave it for the wise and the righteous, and the man who is able rightly to know the causes of things.

After this allegory we have some remarks concerning the treasure and the Philosopher's Stone, and the secret of all secrets, and the gift of God. This is followed by a number of arguments against alchemy, and of course overwhelming arguments in favour of it. Among those who are quoted as alchemists are Plato, Pythagoras, Anaxagoras, Democritus, Aristotle, Morienus, Empedocles, and then, with a delightful disregard of age or country, we read, "Abohaby, Abinceni, Homerus, Ptolemæus, Virgilius, Ovidius." Then digressions on the difficulties of the art, the unity of the art, the art natural and divine; a slight history of the art, in which it is traced back to Adam, although Enoch and Hermes Trismegistus are mentioned as possible founders. A treatise to prove that this art is more certain than other sciences; on the errors of operation; on the principles of the metals; on sulphur; on the nature of gold and silver; and many general remarks on all alchemical subjects. These are the teachings which the *Pretiosa Margarita Novella* pours at the feet of the wise among mankind, by the aid of Paulus Manutius, bearing his father's name of Aldus, and by the grace of the Venetian Senate.

Many attempts were made by the alchemists to explain the origin of the metals; some regarded them as natural compounds of sulphur and mercury, others affirmed that the action of the sun acting upon and within the earth produced them, and that gold was in truth condensed sunbeams; many believed that metals grew like vegetables, indeed it was customary to close mines from time to time to allow them to grow again. Basil Valentine, as we have seen, regarded them as condensations of a "mere vapour into a certain water," by which latter we suspect he meant mercury. Perhaps the most absurd account of the origin of certain things is given by Paracelsus in his treatise, "De Natura Rerum," in the following words, which will show also how utterly nonsensical and unintelligible alchemical language could be, and for that matter very generally was. "The life of metals," he writes, "is a secret fatness; . . . of salts, the spirit of aquafortis; . . . of pearls, their splendour; . . . of marcasites and antimony, a tinging metalline spirit; . . . of arsenics, a mineral and coagulated poison . . . The life of all men is nothing else but an astral balsam, a balsamic impression, and a celestial invisible fire, an included air and a tinging spirit of salt. I cannot name it more plainly, although it is set out by many names."

The peculiarly secret and mystical language which the alchemists adopted was intended to prevent the vulgar from acquiring the results of their long-continued labours. Their language purported to be intelligible to the true adept; but as a rule the alchemists of one age gave various interpretations to one and the same secret communicated by their predecessors. Long recipes for the preparation of the Philosopher's Stone exist, which the authors have generously (as they tell us) given

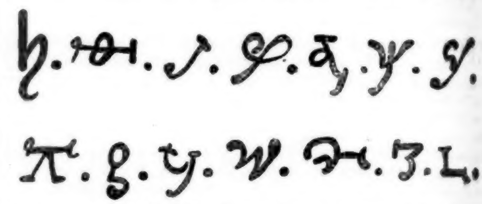

FIG. 16.—Symbols of Lead from Italian MS. of the seventeenth cent

to the world, after much labour, for the benefit of their fellow men. The obscurity of the science was increased by the multiplication of symbols; the presence of which in alchemy clearly points to its connection with astrology and the sister sciences. In time alchemical symbols multiplied almost as much as astrological symbols. In an Italian MS. of the early part of the seventeenth century which we have before us, mercury is represented by 22 distinct symbols, and 33 names, many of which are of distinctly Arabic origin:—such as Chaibach, Azach, Jhumech, Caiban. Lead is represented by the symbols in Fig. 16, and in addition to its ordinary alchemical names, is called Okamar, Syrates, Malochim, and others. The designation of substances as "the green lion," "the flying eagle," "the serpent," "the black crow," and so on, also led to considerable confusion. Both names and symbols were used in a somewhat arbitrary fashion.

It is somewhat strange to think that alchemy should have once received the serious attention of the legislature in this country. In 1404 the making of gold and silver was forbidden by Act of Parliament. It was imagined that an alchemist might succeed in his pursuit, and would then become too powerful for the State. Fifty years later Henry VI granted several patents to people who thought they had discovered the philosopher's stone; and

FIG. 17.—Designs from Mangetus (*Bibliotheca Chemica Curiosa*)

ultimately a commission of ten learned men was appointed by the King to determine if the transmutation of metals into gold were a possibility. We must now leave the subject of alchemy. Those who desire to study it more deeply will find a great mass of matter in the *Bibliotheca Chemica Curiosa* of Mangetus,

* See the first of those Articles.

but if they will take our advice, they will not waste much time in studying the history and progress of a futile and false art.

With Paracelsus (b. 1493, d. 1541), a somewhat new phase of the science of chemistry appeared. By pointing out the value of chemistry as an adjunct to medicine, he caused a number of persons to turn their attention to the subject, and to endeavour to ascertain the properties of various compounds. Thus he helped to withdraw men from the pursuit of alchemy, by asserting that the knowledge of the composition of bodies, which had necessarily been forwarded by alchemy, was of importance to the human race, for the better prevention and curing of their ills. In the way of discovery or research, Paracelsus did little. He mentions zinc and bismuth, and associates them with metallic bodies, and he makes considerable use of several compounds of mercury, and of sal ammoniac. Paracelsus compares the alchemist of his day with the physician, and speaks of the former in the following terms:—"For they are not given to idleness, nor go in a proud habit, or plush and velvet garments, often showing their rings upon their fingers, or wearing swords with silver hilts by their sides, or fine and gay gloves upon their hands, but diligently follow their labours, sweating whole days and nights by their furnaces. They do not spend their time abroad for recreation, but take delight in their laboratory. They wear leather garments with a pouch, and an apron wherewith they wipe their hands. They put their fingers amongst coals, into clay, and filth, not into gold rings. They are sooty and black like smiths and colliers, and do not pride themselves upon clean and beautiful faces."

Among the Paracelsians we find Oswald Crollius, who mentions chloride of silver under the long-retained name of *luna cornea*, or horn-silver, from its peculiar horny appearance and texture after fusion. He was also acquainted with fulminating gold.

The name of Andrew Libavius (died 1616) deserves mention, because he sought to free chemistry from the mazes of alchemy and mysticism in which it was involved. In this he to some extent succeeded; and he appears also to have been a patient worker in the field of the science which he did so much to promote. He discovered the perchloride of tin which is even now called *fuming liquor of Libavius*; he also proved that the acid (sulphuric acid) procured by distilling alum and sulphate of iron, is the same as that prepared by burning suphur with saltpetre. Libavius was great at the making of artificial gems, and was able to imitate almost any precious stone by colouring glass with various metallic oxides.

G. F. RODWELL

SCIENTIFIC SERIALS

THE *Zoologist* continues Dr. J. E. Gray's catalogue of the whales and dolphins inhabiting or incidentally vsiting the seas surrounding the British Isles.—The Rev. A. C. Smith gives the results of the observations of Dr. Rey, of Halle, on the colouring of cuckoos' eggs, which are in favour of Dr. Baldamus theory.—From notes by Mr. J. Sclater and Mr J. Gatcombe, from Castle Eden and Plymouth, we find that the glaucous gull has been obtained in both places, and the winds have driven ashore several other sea-birds, petrels, &c.

THE *Monthly Microscopical Journal* commences with the excellent address of the president of the Microscopical Society, the perusal of which, from the enthusiasm exhibited will convince sceptics that there is a fund of enjoyment in science equal to that in other mental occupations.—Mr. Parker also contributes a paper on the development of the skull in the thrushes.—The Rev. S. L. Brackey has a paper on reduced apertures in immersion objectives, a subject on which Mr. R. B. Tolles and Mr. F. H. Wenham have a correspondence.—There is a short and severe review of Dr. Bastian's "Beginnings of Life."—Mr. S. Wells has a paper on the structure of *Eupodiscus Argus*, and G. W. Royston-Pigott one on spurious appearances in microscopic research.—Captain T. H. Lang gives a short abstract of Prof. Smith's "Conspectus of the Diatomaceæ," which has appeared in the *Lens*.

PETERMANN'S *Mittheilungen* (19 Band, 1873, ii). The first paper is another contribution to the literature of North Polar Exploration by J. Spörer, in which he shows the importance to science and humanity of records of exploration. One of the maps in this number shows the route followed by two Russians, Pawlinow and Matusowski, in their politico-commercial expedition of 1870, in Western Mongolia. Herr Fricke, a German merchant who has extensive connections both in East and West Africa, writes, giving several interesting details concerning the state of trade with the interior of South Africa, both from the east and west coast, showing that European connections with the interior extend much further than is indicated in our geographies and maps.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 13.—"Note on Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S.

"Visible Direction: being an Elementary Contribution to the Study of Monocular and Binocular Vision." By James Jago, M.D. Oxon., A.B. Cantab., F.R.S.

Anthropological Society, March 11.—At this, the first meeting of this Society, the rules proposed by the Organising Committee were adopted, subject to confirmation at the first Annual General Meeting; and the following officers were elected:—President—Dr. R. S. Charnock, F.S.A. Vice-Presidents—Capt. R. F. Burton, F.R.G.S., and C. Staniland Wake. Treasurer—Joseph Kaines. Council—Dr. J. Beddoe, H. B. Churchill, Dr. Barnard Davis, F.R.S., John Fraser, Dr. G. Harcourt, Dr. Sinclair Holden, Dr. T. Inman, Dr. Kelburne King, Dr. J. Barr Mitchell, and T. Walton. Hon. Sec.—A. L. Lewis. Hon. For. Sec.—Dr. Carter Blake. This Society has been founded in consequence of a difference of opinion among the members of the Anthropological Institute, and a letter from Capt. Burton, the well-known traveller, heartily supporting the new organisation, was read.

Geologists' Association, March 7.—Henry Woodward, F.G.S., &c., president, in the chair.—"On the Geology of Brighton," by Mr. James Howell. Surface indications did not, he believed, afford evidence that the northern portion of the Downs had been submerged since its upheaval. Historical documents, submerged forests, and the shallowness of the sea's bottom, afforded abundant proof of the great encroachment of the sea along this part of the coast of Sussex during the historic period. The site of Old Brighton was stated to be seaward between East and West Streets, and not, as Lyell states, where the chain-pier now stands; and the coast line at the period when the Brighton Valley was an estuary of the sea and a river, was very different from what it is now. The geological formations at Brighton were stated to be six, viz. silt in the valley, brick-earth of Hove, the Elephant-bed, Templefield deposit, plastic clay of Furze Hill, and the upper chalk. The present paper embraced Mr. Howell's observations of the first three. In the lower portion of the silt and the coombe rock beneath it, are embedded immense numbers of water-rolled sandstones, similar to the sarsenstones distributed over the surface of the downs; but whether of Wealden or Tertiary origin is unknown. The brick-earth is a later formation than the elephant-bed upon which it everywhere rests, though the fossiliferous remains embedded in it are the same, viz., those of the mammoth, horse, red-deer, whale, and shells of an Arctic type. If, as Mr. Godwin Austen tells us, brick-earth is the wash of a terrestrial surface, how are we to account for the marine remains embedded in it? The pebbles of Palæozoic rocks, found in the old sea-beach under the elephant-bed, were stated to have come from France, when that country was united to Britain, having travelled along a beach once extending from Brighton to Calvados. The observations of Mr. Howell, of how pebbles and pieces of rock travel along a coast, aided by sea-weed to which they may be attached, supported this opinion. The author in conclusion opposed the opinion entertained by the geological section of the British Association during their visit to the Kemp Town section of the elephant-bed, that this remarkable deposit was formed by ice-action, and adduced the fact that the materials composing it are all water-rolled as corroborating the opinions of Webster, Mantell, and Lyell.

DUBLIN

Royal Geological Society, Jan. 8.—Professor Macalister, president, in the chair.—The Rev. Dr. Haughton, F.R.S., read a paper on Stirm's Fertiliser, from New Hampden, U.S.—Rev. Maxwell Close read some Notes on the High Level Gravels near Dublin.

Feb. 12.—This was the annual meeting. The outgoing president, Professor Macalister, delivered the annual address,

selecting for the theme of his discourse, the subject of Micropetrography—a subject in which a vast amount of work remains to be done—one, it is feared, not so much known as it deserves to be, and much misunderstood. Workers are needed to follow up the lines of research of Rosenbusch, Lasault, and Fuchs, who are working out the correlation of petrography and petrology, the structure in regard to the position of the rock mass. As an an appendix to his address, Prof. Macalister gives an important bibliography of the subject of Microgeology. Prof. E. Hull, F.R.S., was elected **President** for the ensuing session.

PARIS

Academy of Sciences, March 10.—M. de Quatrefages, president, in the chair.—M. Berthelot took his seat as a member of the Academy. The following papers were read:—On Father Secchi's new hypothesis by M. Faye. The author replied to the Rev. Father's late note by proving that he had first stated that the solar spots were craters of eruption, and next that they were not eruptions, but were caused by them being in fact the erupted matter cooled by its passage above the chromosphere, the faculæ being the centres of eruption.—M. Faye showed this to be incompatible with the observed facts, the spots being surrounded with faculæ, whereas, according to Secchi's last theory, they ought to surround faculæ.—On the circulation of solar hydrogen, with an answer to some remarks by M. Tacchini, by M. Faye, treated of the spot phenomena; the author thinks that hydrogen is drawn down by the cyclones and returns to the surface around them. He also suggested that D_3 would probably be found to belong to a very rarified hydrocarbon of the acetylene series.—On the density of the vapour of phosphoric chloride, by M. Wurtz, who found that when precautions were taken, to prevent dissociation, the normal two volumes was occupied, experiment giving 7·226, and theory requiring 7·217.—On the springs of the Seine basin by M. Belgrand.—Researches on the action of the tympanic chord on the circulation in the tongue, by M. A. Vulpian.—On the industrial production of cold by the detention of permanent gases and of air in particular, by M. Armengaud.—On the production of silent electric discharges and on their mode of action, by M. Boillot.—Experiments on putrefaction, &c., by M. Laujorrois.—On the assimilability of phosphates, by M. H. Joulie.—A note on the unity of force and of matter, by Madame C. Royer.—On the theory of solar spots, by M. Tacchini, was an answer to M. Faye. The author contends that the hydrogen carried down by the cyclones ought to return up their axes, and not around them; and as this is not the case, he thinks that his criticisms remain unrebutted.—A paper on the trajectories of points, &c., by M. Mannheim.—On benzylated naphthalin, by M. Ch. Frotté. The body in question is produced by the action of benzylic chloride on naphthalin in the presence of powdered zinc.—On a combination of urea with acetylic chloride, by M. D. Tommasi. An atom of hydrogen in urea is replaced by the acetyl compound.—On the composition of guanos, &c., by M. A. Baudrimont.—On asphyxia and on the causes of the respiratory movements in fishes, by MM. Gréhant and Picard.—On the influence of ammonia in manufactories where mercury is employed, by M. J. Meyer. The author asserts that he has succeeded in stopping all the terrible effects of mercurial poisoning in the silvering rooms of the Saint Gobain glass works by watering the floors every evening with half a litre of commercial ammonia. He states that since 1868 he has not had a single workman attacked.—On the saccharine matter of mushrooms, by M. A. Müntz.—On the normal microzymes of milk as the cause of the coagulation and alcoholic, acetic, and lactic fermentations of that liquid, by M. A. Béchamp.—On the quaternary fossils collected by M. Œlert at Louverné (Mayenne), by M. A. Gaudry.—On the existence of man in Alsace during the glacial epoch, by M. Ch. Grad.—On the movements of the atmosphere as regards the prediction of weather, by M. de Tastes.—On the use of vermouth, by M. E. Decaisne. The author thinks that the use of this liquid as a drink ought to be abandoned.

DIARY

THURSDAY, MARCH 20.

ROYAL SOCIETY, at 8.30.—On the Distribution of Vertebrata in Relation to the Theory of Evolution: Dr. J. D. Macdonald.—On the Temperature at which Bacteria, Vibriones, and their supposed Germs are killed when immersed in Fluids or Exposed to Heat in a Moist State: Dr. Bastian.—Some New Theorems on the Motion of a Body about a Fixed Point: E. J. Routh.

SOCIETY OF ANTIQUARIES, at 8.30.—On the Hunnebedden of Drenthe in the Netherlands; Miscellaneous Antiquities: Governor Gregory.

ZOOLOGICAL SOCIETY, at 4.

LINNEAN SOCIETY, at 8.—On the "Take-all" Corn Disease of Australia: Dr. Mücke.

CHEMICAL SOCIETY, at 8.—On Iron and Steel: C. W. Siemens.

ROYAL INSTITUTION, at 3—The Chemistry of Coal and its Products: A. V. Harcourt.

FRIDAY, MARCH 21.

ROYAL INSTITUTION, at 9.—On the Mythology of India: E. D. Lyon.

OLD CHANGE MICROSCOPICAL SOCIETY, at 6.30.—Annual Meeting.

ROYAL COLLEGE OF SURGEONS, at 4.—Extinct Mammals: Prof. Flower.

QUEKETT CLUB, at 8.—Conversazione.

SATURDAY, MARCH 22.

ROYAL INSTITUTION, at 3.—Darwin's Philosophy of Language: Max Müller.

SUNDAY, MARCH 23.

SUNDAY LECTURE SOCIETY, at 4.—The Theory of Stringed Musical Instruments: W. H. Stone.

MONDAY, MARCH 24.

GEOGRAPHICAL SOCIETY, at 8.30.—Notes on Khiva, and Routes leading to that country: Major-Gen Sir H. C. Rawlinson, K.C.B., President.

LONDON INSTITUTION, at 4.—Fungoid Organisms: Prof. Thiselton Dyer.

ROYAL COLLEGE OF SURGEONS, at 4.—Extinct Mammals: Prof. Flower.

TUESDAY, MARCH 25.

ROYAL INSTITUTION, at 3.—Forces and Motions of the Body: Prof. Rutherford.

WEDNESDAY, MARCH 26.

ROYAL COLLEGE OF SURGEONS, at 4.—Extinct Mammals: Prof. Flower.

LONDON INSTITUTION, at 7.—Courts of Special Commercial Jurisdiction: N. H. Paterson.

SOCIETY OF ARTS, at 8.—On the Edible Starches of Commerce: P. L. Simmonds.

GEOLOGICAL SOCIETY, at 8.—Synopsis of the Younger Formations of New Zealand: Capt. F. W. Hutton.—On the Tree-ferns of the Coal-measures and their Relations to other living and fossil forms: W. Carruthers.—Notes on the Geology of Kazirûn, Persia: A. H. Schindler.

ARCHÆOLOGICAL ASSOCIATION, at 8.

ROYAL SOCIETY OF LITERATURE, at 8.30.—The Rhodian Law, and its connection with the Laws of Mediæval Europe: W. S. W. Vaux.

SOCIETY OF TELEGRAPH ENGINEERS, at 7.30.—On a new method of testing short lengths of highly Insulated Wire: Prof. Fleeming Jenkin.—On some points in connection with the India Government Telegraphs: W. E. Ayrton.

THURSDAY, MARCH 27.

ROYAL INSTITUTION, at 3.—Coal and its Products: A. V. Harcourt.

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTQUARIES, at 8.30

BOOKS RECEIVED

ENGLISH.—A Manual of Metallurgy: G. H. Makins (Ellis & White).—The Atmosphere Ed. by J. Glaisher (Sampson & Low)—The University Oars: Dr. J. E. Morgan (Macmillan).—Chaureau's Comparative Anatomy of the Domesticated Animals. 2nd edit. Translated by G. Fleming (Churchill). The Scientific Bases of Faith: J. J. Murphy (Macmillan).—Mensuration: D. Munn (Chambers).—Essay on the Physiology of the Eye: S. H. Salom (S. H. Salom).—The Year Book of Facts, 1873: J. Timbs (Lockwood)—Steam in the Engine; its Heat and its Work: P. Kauffer (Blackie).—Results of Meteoroogical Observations made in the Royal Observatory, Cape of Good Hope: Sir Thos. Maclear (Solomon).—Chemistry for Schools; an Introduction to the Practical Study of Chemistry. 2nd edit: C. H. Gill (Stanford).—Handbook for the Physiological Laboratory; Brunton-Foster, Klien, and Sanderson.

AMERICAN.—One Law in Nature: Capt. H. M. Lazelle (D. V. Nostrand, New York).

PAMPHLETS RECEIVED

AMERICAN.—United States Commission of Fish and Fisheries, Pt. 1. Report on.—Condition of the Sea Fisheries of the South Coast of New England in 1871–2: S. F. Baird.—On the Glacial and Champlain Eras in New England: J. Dann.—Proceedings of the Academy of Natural Sciences, Philadelphia.

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